PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCII.

PART I.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.
MDCCXCII.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds

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of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1791, two Medals on Sir GODFREY COPLEY's Donation; one to JAMES RENNELL, Esq. F.R.S. for his Paper on the Rate of Travelling as performed by Camels, printed in the last Volume of the Philosophical Transactions; the other to JOHN ANDREW DE LUC, Esq. F.R.S. for his Improvements in Hygrometry.

ERRATUM.

P. 68, l. 23. *for* 2,1446 — 60,08711 *read* 2,14466 — 0,08711.

PHILOSOPHICAL

TRANSACTIONS.

I. *On the Ring of Saturn, and the Rotation of the fifth Satellite upon its Axis.* By William Herschel, *LL.D. F.R.S.*

Read December 15, 1791.

It is well known to Astronomers that the ring of Saturn becomes alternately enlightened on one of its sides, and that this change of illumination takes place when the planet passes through the node of the ring. This happened in October, 1789, when the southern plane, which had been in the dark for about fifteen years, became visible to us: an event to which I have looked forwards with considerable impatience. In the year 1790, the position of the ring was still too oblique to permit me to examine it well enough to form a proper judgment of its appearance, but lately I have been able to view it to greater advantage, with every one of my telescopes.

In a former paper,* where I ventured to hint at a division of the ring of Saturn, it was highly necessary to express that surmise with proper doubts concerning the reality of so won-

* Phil. Trans. Vol. LXXX. page 4.

derful a construction; but my late views of its southern plane, assisted by some conclusions drawn from the discovery of the quick rotation of the ring, have enabled me to speak decisively on this subject. My suspicion of a divided or double ring arose chiefly from the following circumstances.

In the first place, the black belt, during the time of about ten years that I observed it, on the northern plane, was subject to no kind of change; but remained always, permanently, of the same breadth and colour. With regard to its breadth, it is true that I could only judge of that part of it which goes across the body of the planet, by the rules of perspective, which made me suppose it to be as broad there as it was on the two sides; yet now, as we know that the ring revolves in about ten hours and a half, it is very certain, that the apparently narrow part, across the body, and that which was hidden behind the planet, in the course of an evening, when I have been observing Saturn for many hours together, must have been exposed to view in their full breadth, upon the sides of the ring; and that, if there had been any difference, I must have perceived it; especially as I was continually on the look out for such phænomena, by way of ascertaining, if possible, the rotation of the ring.*

In the next place, the colour of this dark belt was also uniformly the same, whenever I observed it under equally favourable circumstances; and being so well defined on both its

* When I say that the black division was always of the same breadth, I do not mean to exclude very small variations, not only of the breadth of the black mark, but of the ring itself, which I have occasionally observed, and which it may be necessary, hereafter, to communicate at full length; but these, almost imperceptible, differences might arise from causes that are foreign to our present purpose.

borders, and, in every part of the revolving ring, presenting us with the same view of colour, breadth, and sharpness of its outlines, no kind of hypothesis but a division of the ring, through which the open heavens may be seen, will answer the conditions of this phænomenon. It remained therefore only to ascertain, whether the southern plane would present us with the same aspect. And since I have lately had a great number of fine views of the ring of Saturn, I shall here deliver as many of the observations as will be sufficient to throw light enough on the subject, to enable us to decide the question, whether this ring be double or single?

Observations on the Ring of Saturn.

Sept. 7, 1790. 20-feet reflector. No dark division can as yet be seen upon the ring of Saturn; but it is hardly open enough to expect it to be visible.

Aug. 5, 1791. 20-feet reflector. The black list, on this side of the ring of Saturn, is exactly in the same relative place where I saw it on the northern plane.

Sept. 25, 1791. 20-feet reflector. The black division goes all around the ring, as far as I can trace it, exactly in the same place where I used to see it on the north side.

Oct. 13, 1791. 10-feet reflector. The black division upon the southern plane of Saturn's ring, is in the same place, of the same breadth, and at the same distance from the outer edge, that I have always seen it on the northern plane. With a power of 400, I see it very distinctly; it is of the same kind of colour as the space between the ring and the body, but not so dark.

Oct. 24, 1791. 7-feet reflector. With a new, machine-

polished, most excellent speculum, I see that the division on the ring of Saturn, and the open spaces between the ring and the body, are equally dark, and of the same colour with the heavens about the planet.

20-feet reflector. The black division upon the ring is as dark as the heavens. It is equally broad on both sides of the ring. I see it very steadily, and can trace it a good way towards Saturn; both on the part of the ring which is turned towards us, and on that which lies the other way. I trace it as far as the place where a line, perpendicular to the direction of the ring, would touch the inside of the ring, or the outside of the open space between the ring and the body of the planet.

40-feet reflector. I see the division on the ring of Saturn of the same colour as the surrounding heavens. It is of an equal breadth on both sides, and I can trace it a great way towards the body of Saturn.

20-feet reflector. With a power of 600, I can trace the division very nearly as far as the place, where a perpendicular to the direction of the ring, would divide the open space between the planet and the ring, into two equal parts.

From these observations, added to what has been given in some former papers, I think myself authorized now to say, that the planet Saturn has two concentric rings, of unequal dimensions and breadth, situated in one plane, which is probably not much inclined to the equator of the planet. These rings are at a considerable distance from each other, the smallest being much less in diameter at the outside, than the largest is at the inside.

The dimension of the two rings and the intermediate space are nearly in the following proportion to each other.

Inside diameter of the smallest ring	5900 parts.
Outside diameter - - -	7510
Inside diameter of the largest ring	7740
Outside diameter - - -	8300
Breadth of the inner ring -	805
Breadth of the outer ring - -	280
Breadth of the vacant space -	115

Admitting, with M. DE LA LANDE, that the breadth of the whole ring, as formerly supposed to consist of one entire mass, is near one third of the diameter of Saturn, it follows that the vacant space between the two rings, according to the above statement, amounts to near 2513 miles.

In giving these proportions, which are merely taken from very accurate representations of the phænomena that offered themselves, I do not mean to be scrupulously exact, but reserve a greater accuracy for a future opportunity; when a micrometer, which I have lately applied to the 40-feet telescope, will assist me to have recourse to proper measures.

It may be remarked, that this opening in the ring must be of considerable service to the planet, in reducing the space that is eclipsed by the shadow of the ring to a much smaller compass; both on account of the direct light it lets through, and because there will be a strong reverberation of the rays of the sun between the two opposite edges. Moreover, if these rings should be surrounded by some atmosphere, which is highly probable, the refractions that will take place upon the edges will still contribute to lessen the darkness which the shadow of an undivided ring would have occasioned.

As we have now admitted Saturn to have two rings entirely detached from each other, so as plainly to permit us to

see the open heavens through the vacancy between them; and as in my former paper I have given the revolution of the ring, which was then supposed to be all in one united mass, it will be necessary to examine, whether both rings partake in the same revolution, or to which the period which has been assigned belongs?

To decide this point, we must recur to the observations of the spots by which the rotation of the ring was determined. The spot called α,* for instance, which has been observed to revolve with great regularity through upwards of 300 periods, between the 28th of July and the 24th of December, 1789, was certainly situated pretty near the outer edge. The spot β, as may be gathered from the observation of the 16th of September, and 25th of December, was most likely on the very edge itself; nor could the spot δ be far from it. This, without considering the situation of γ and ϵ, is quite sufficient to determine us to assign the period we have given to belong to the large, thin and narrow, outward ring.

The spots γ and ϵ were probably at some distance from the outward edge of the outer ring; but this distance might possibly not exceed that of the inside edge of the same ring. We may however admit them to have adhered to the inner ring, whose rotation is perhaps not very different from that of the outer one; or we may examine whether these two spots may not perhaps agree to some other supposed revolution of the inner ring; but then the observations that are given of them will hardly be sufficient for establishing the time of that ring's rotation with accuracy, though they undoubtedly must amount to a proof that it also revolves with great volocity on its axis.

* See Phil. Trans. Vol. LXXX. page 481.

That there should be a small difference in the periods of the rotation of the two rings, is highly probable from their different dimensions; and now, that the rotation is known, the division of it into two parts seems to be a very natural consequence of its construction. For, when the extreme thinness is taken into consideration, we find by KEPLER's law, of the periods of revolving bodies placed at different distances, that it would be very wonderful for so thin, and so broad a plane, to have adhesion enough to keep together; and that consequently this ring in its divided state, supposing the rotation of the parts to favour the construction, is more permanent than it would be otherwise. This however is only mentioned as a collateral circumstance, and by no means intended either as a proof of the division, or the different rotation of the two parts of the ring. For, notwithstanding we cannot but set the highest value upon the excellent theories that have been lately delivered in the Memoirs of a learned Society, of which I also have the honour to be a member, we must refer entirely to observation for the necessary data on which to found our subsequent computations.

The memoir to which I allude * refers to observations of many divisions of the ring of Saturn. This must lead us to consider the question, whether the construction of this ring is of a nature so as permanently to remain in its present state? or whether it be liable to continual and frequent changes, in such a manner as in the course of not many years, to be seen subdivided into narrow slips, and then again as united into one or two circular planes only? Now, without entering into a discussion, the mind seems to revolt, even at first sight,

* See *Histoire de l'Académie Royale des Sciences de Paris*, 1787, page 249.

against an idea of the chaotic state in which so large a mass as the ring of Saturn must needs be, if phænomena like these can be admitted. Nor ought we to indulge a suspicion of this being a reality, unless repeated and well-confirmed observations had proved, beyond a doubt, that this ring was actually in so fluctuating a condition. Let us therefore examine what facts we have to guide us in this inquiry.

After looking over all my observations upon Saturn, since the year 1774 to the present time, I can find only four where any other black division upon the ring is mentioned than the one which I have constantly observed, and from which I have deduced the actual division of the ring into two very unequal portions. These observations are as follows.

June 19, 1780. 10^h 15′ mean time. With a new 7-feet speculum, having an aperture of 6,4 inches, with also a much improved small speculum, and a power of about 200. I see a second black list upon the ring of Saturn, close to the inner side, on the preceding arm of the ring. See figure 1. Tab. I.

June 20, 1780. 10^h 10′. I see the same double list on the preceding side of the ring.

June 21, 1780. 10^h 1′. Small 20-feet, Newtonian reflector, power 200. I see the second black list on Saturn's ring. It is closer to the inside than the other is to the outside; but it is only visible on the preceding side of the ring. See figure 2.

June 26, 1780. 9^h 34′. Small 20-feet, Newtonian reflector; aperture confined to 7 inches. The 2d black list, on the preceding side of the ring of Saturn, is visible.

June 29, 1780. 10^h 19′. Saturn's belts are very clear. I see but one black list upon the ring. The shadow of the planet

is visible upon the side of the ring, as well as upon the small northern part that projects beyond the planet. See fig. 3.

Nov. 21, 1791. 0h 28′ sid. time. 40-feet reflector, power 370. There is no other black division visible upon the ring of Saturn but the one near the outer edge.

It must be confessed that Saturn was in the very best situation for viewing the plane of the ring, when the first four observations were made; and that consequently they may be looked upon as a strong evidence for another division. But hitherto I have set them aside as wanting more confirmation, not only because I could never perceive the same dark line on the following side of the ring as well as on the preceding side; nor since I could not find it on the 29th of June, 1780, as we have seen above; but chiefly, because I have not been able, with any of my best instruments, to see it again at all. We also find by the observation of the 21st of November, 1791, which has been added, that the southern plane, as yet, presents us with no other division than the capital one, which I have observed these thirteen years, on both sides of the ring. However, if the opening should be very narrow, and the rings eccentric, it is possible that a dark line might by this means become visible on one side only. Moreover, these objects may be so minute, that no other time than when the plane of the ring is exposed as much as it can possibly be, will do to ascertain such phænomena. This will happen again about the year 1796, when we may hope to have a satisfactory view of it, with our large instruments.

It remains now to consider the observations that have been made by M. Cassini, Mr. Short, and Mr. Hadley.

Without being in possession of the original observations of

M. CASSINI, I cannot decide whether the black list which he saw was the same which I have observed. M. DE LA LANDE says (*Ast.* Vol. III. page 441.) that CASSINI saw it divided by a small black line into two *equal* parts. M. DE LA PLACE (*Mémoire sur la Théorie de l'Anneau de Saturne*) mentions that CASSINI saw the breadth of the ring divided into two parts *almost* equal. It should seem from this, that M. CASSINI was not particularly attentive to the proportions of the division; in which case his observations and mine will agree perfectly well; but if he has any where expressly mentioned, that the ring was divided into *equal* parts, so that we may be certain he was particularly attentive to that circumstance, it will follow evidently that the ring, since his time, has undergone a very capital change in its construction.

Mr. SHORT assured M. DE LA LANDE, that he had seen many divisions upon the ring, with his telescope of 12-feet. A thing of such consequence, and so new, ought certainly to have been given in a more satisfactory and circumstantial way than only by communicating it, from memory, in conversation, to another person. Besides, it is well known that many telescopes will give double and treble images, and that especially those which have large apertures are subject to tremors, which multiply small lines. For these reasons, we can hardly take into account observations that seem not to be sufficiently established. What has been said is, however, by no means intended to undervalue Mr. SHORT's observations; and this, I hope, will be evident, when it is remembered how scrupulously I have just before set aside four of my own, because I looked upon them as not sufficiently confirmed.

Mr. HADLEY's observation of the division of the ring, with a

$5\frac{1}{2}$-feet Newtonian reflector, which was certainly a very excellent instrument, agrees perfectly well with mine.

From what has been said, it does not appear to me that there is a sufficient ground for admitting the ring of Saturn to be of a very changeable nature, and I guess that its phænomena will hereafter be so fully explained, as to reconcile all observations. In the mean while, we must withhold a final judgment of its construction, till we can have more observations. Its division, however, into two very unequal parts, can admit of no doubt; and the following are measures taken of the diameter of the largest or outward ring.

Oct. 7, 1791. *Correction of the 20-feet clock* — 2′ 16″,5.

Measures of the ring of Saturn with the 20-feet reflector.

0h 37′	1st measure	54″,115
	2d ———	52 ,537
	3d ———	52 ,875
	4th ———	54 ,679
	5th ———	52 ,903
	6th ———	53 ,044
	7th ———	53 ,411

53″,366 mean of the seven measures.

Proper allowance was made for the wires being tangents to the outside of the ring.

When this measure is reduced to what it would be at the mean distance of Saturn from the earth, we have 46″,832.

Oct. 24, 1791. *Correction of the 40-feet clock* + 25″,4.

Measure of the ring of Saturn with the 40-feet reflector. Power 370.

$1^h\ 3'$	1st measure	53″,914
	2d ———	53 ,260

53″,587 mean of the two measures.

Reduced to the mean distance of Saturn, the measure is 47″,241.

Nov. 21, 1791. *Correction of the 40-feet clock* — 7″,8.

Another measure of the ring of Saturn with the 40-feet reflector. Power 370.

$0^h\ 48'$	1st measure	50″,627
	2d ———	50 ,042
	3d ———	50 ,808

50″,492 mean of the three measures.

Reduced to the mean distance of Saturn the measure is 45″,803.

Oct. 24	47″,241
Nov. 21	45 ,803

46″,522 mean of the measures with the 40-feet reflector.

40-feet	46″,522
20-feet	46 ,832

46″,677 mean of all the measures.

By way of forming more easily a comparative idea of the stupendous size of this ring of Saturn, I have calculated the proportion it bears to the earth, and find that its diameter is to that of the latter as 25,8914 to 1; and that consequently,

when seen at the mean distance of the sun, it will subtend an angle of 7′ 25″,332.

From the above proportions we also compute that this ring must be upwards of 204883 miles in diameter.*

On the Rotation of the fifth Satellite of Saturn, on its Axis.

In my frequent observations of the Saturnian system, I remarked, that the 5th satellite is subject to a change of brightness. This having been noticed before by other observers, I did not at first pay so much attention to it as I soon afterwards found this circumstance deserved. When I saw this satellite always assume the same brightness in the same part of its orbit, and perceived that its change was regular and periodical, it occurred to me very naturally, that the cause of this phænomenon could be no other than a rotation upon its axis. It became necessary therefore to find out a method to determine the time of this rotation.

In order to investigate this, I pursued the satellite with great attention, and marked all its changes of apparent brightness. The result of many observations is as follows. The light of the satellite is in full splendour during the time it runs through that part of its orbit which is between 68 and 129 degrees past the inferior conjunction. In passing through this arch it does not fall above one magnitude short of the brightness of the 4th satellite. On the contrary, from about 7 de-

* In this calculation I have used for the earth's diameter the sum of the longer and shorter semi-axes which are given in Mr. Dalby's Paper, published in the last volume of the Philosophical Transactions. If we compute the vacant space between the two rings immediately from the above dimensions of the outward ring, we shall have 2839 miles; and this will certainly be more accurate than the result which has been drawn from the proportion of the breadth of the ring to the diameter of Saturn.

grees past the opposition till towards the inferior conjunction, it is not only less bright than the 3d, but hardly, if at all, exceeds the 2d, or even the 1st satellite; provided the latter be then about its greatest elongation, where its light is least impeded by the brightness of the planet. Upon the whole, the alteration seems to amount to what among the fixed stars, and with the naked eye, would be called a change from the 5th to the 2d, and from the 2d to the 5th magnitude.

Having thus observed this satellite, for many of its revolutions round the primary planet, to lose and regain its light regularly, it is evident that the time of its rotation on its axis cannot differ much from that of its revolution round Saturn. I think myself sufficiently authorized to make this conclusion, notwithstanding it may have happened sometimes that the light of the satellite has suffered an occasional change, of short duration, from other causes; for the same reason that we should certainly allow those who first saw the spots in the sun to be in the right to assign the period of its rotation *nearly*, when they perceived that the same spot made several revolutions, notwithstanding that spot might afterwards vanish. But I may go farther, and ascertain upon sufficient grounds, that this satellite turns once upon its axis, exactly in the time it performs one revolution round its primary planet. This degree of accuracy is obtained by taking in the observations of M. CASSINI, which are related in the *Mémoires de l'Académie des Sciences*, 1705, page 121; where we find it mentioned, that "the 5th satellite of Saturn disappears regularly for about one "half of its revolution, when it is to the east of Saturn." The same memoir contains also a conjecture of this satellite's rotation upon its axis; but this surmise is contradicted as prema-

ture, in 1707, page 96; where we find the following paragraph. "M. CASSINI gives an example of the danger there is "in these sort of determinations, that are made too hastily. "The 5th satellite of Saturn, of which we have said, in the "History of 1705, page. 121, that it grew invisible, in the "eastern half of the circle it describes about Saturn, began, "in the month of Sept. 1705, to be there visible, as well as in "the western half, where it always was so. Hence the con-"jectures which we have related cease to be well founded."

Now, without determining whether the satellite, from some cause or other, ceased to change its brightness, or whether its phænomena were not sufficiently followed to come to a proper conclusion, I think that with the assistance of observations at so great a distance of time as those of M. CASSINI, I may sufficiently establish the period of this satellite's rotation. For since I have traced the regular, and periodical change of light, through more than ten revolutions, and find them, in all appearance, to be contemporary with its return about Saturn, it leads us directly to a strong presumption that its rotation upon its axis, like that of our moon, strictly coincides with its revolution round its primary planet; and the observations of M. CASSINI completely confirm this conclusion. For, had he seen the satellite brightest in any other part of its orbit, our observations would not have agreed together; but since the year 1705, the satellite has made about 397 revolutions; and yet the phænomena described by CASSINI answer now as exactly to my own observations, as the spots in our moon, viewed in CASSINI's time, answer to those we now observe.

If it should be objected, that the 5th satellite of Saturn has not been continually observed, and that consequently these ap-

pearances might either not happen at all, or fall upon different places in its orbit; I answer, that a period of more than ten revolutions, which I have included, is already a strong argument that no such change has taken place; for if the satellite had but made a single rotation upon its axis more or less than it has made revolutions round Saturn, the change must amount to nearly one degree *per* revolution; that is, to about ten degrees during the time of my taking notice of it; which is a quantity I think I might have perceived. However, to remove all doubt, we have some valuable observations of M. BERNARD, who in the year 1787, also found the 5th satellite of Saturn subject to the same change of light that M. CASSINI had observed.* Now, by joining those to mine, we have a short period of near 20 revolutions that agree together, so as to preclude all doubt of any intermediate change; and therefore we cannot be liable to err, when we extend this period to all the 397 revolutions since CASSINI's time, and by that means ascertain that the 5th satellite of Saturn turns upon its axis, once in 79 days, 7 hours, and 47 minutes.

I cannot help reflecting, with some pleasure, on the discovery of an analogy, which shews that a certain, uniform plan is carried on among the secondaries of our solar system; and we may conjecture, that probably most of the moons of all the planets are governed by the same law; especially if it be founded on such a construction of the figure of the secondaries, as makes them more ponderous towards their primary planets. For, if even the 5th satellite of Saturn, which is at so great a distance from its planet, is affected by such a law, of course the other satellites are not very likely to have escaped its influence.

* See *Mémoires de l'Académie*, 1786, page 378.

From the considerable change in the brightness of the 5th satellite of Saturn, we may be certain that some part of its surface, and this by far the largest, reflects much less light than the rest; and, from the points of its orbit in which it appears brightest to us, we conclude that neither the darkest nor brightest side of the satellite is turned towards the planet, but partly one and partly the other; though probably rather less of the bright side.

The great regularity of this change of brightness seems to point out another resemblance of this satellite with our moon. It is well known that we see the spots of the moon pretty nearly of the same brightness, so as not to be overcast in a very strong degree by dense clouds to disfigure them, and therefore have great reason to surmise that her atmosphere is extremely rare; which indeed we also know from other principles: In like manner, on account of the uninterrupted changes in the brightness of the 5th satellite of Saturn, we may suppose that it also partakes of a similar fate with respect to its atmosphere, which is probably as rare as that of our moon.

On the Distance of the fifth Satellite.

The distance of the 5th satellite from Saturn is allowed to be the most proper for obtaining a true measure of the quantity of matter contained in the planet; for which reason I have taken many measures of it with the 20-feet reflector. I give them at full length, that the validity of them may appear in its proper light.

Sept. 25, 1791. *Correction of the clock* — 2′ 19″,5 *for midnight.*
Distance of the 5th satellite of Saturn from the centre of the

planet; measured with the 20-feet reflector, and a magnifying power of 157.

23^h	4′	1st measure	8′ 55″,684
23	19	2d ———	8 53 ,175
23	33	3d ———	8 59 ,179
23	47	4th ———	8 52 ,123
23	55	5th ———	8 56 ,361
0	2	6th ———	8 55 ,797

8′ 55″,5 mean of the six measures.

Sept. 26, 1791. *Correction of the clock* — 2′ 19″,8.

23^h	15′	1st measure	9′ 3″,745
23	25	2d ———	9 2 ,758
23	31	3d ———	9 7 ,014
23	38	4th ———	9 6 ,592
23	42	5th ———	9 8 ,001
23	45	6th ———	9 6 ,479

9′ 5″,8 mean of the six measures.

Sept. 27, 1791. *Correction of the clock* — 2′ 20″,0.

22^h	53′	1st measure	9′ 20″,656
22	57	2d ———	9 20 ,359
23	1	3d ———	9 20 ,149
23	5	4th ———	9 20 ,641
23	11	5th ———	9 20 ,064
23	16	6th ———	9 20 ,840

9′ 20″,5 mean of the six measures.

Sept. 28, 1791. *Correction of the clock* — 2′ 20″,2.

21ʰ 38′	1st measure	9′ 27″,759	
21 44	2d ——	9 29 ,957	
21 46	3d ——	9 27 ,815	
21 50	4th ——	9 28 ,238	
21 53	5th ——	9 28 ,970	
21 58	6th ——	9 27 ,420	
		9′ 28″,4	mean of the six measures.

Sept. 29, 1791. *Correction of the clock* — 2′ 20″,4.

0ʰ 17′	1st measure	9′ 37″,060	
0 22	2d ——	9 36 ,552	
0 26	3d ——	9 36 ,270	
0 29	4th ——	9 36 ,862	
0 33	5th ——	9 37 ,765	
0 36	6th ——	9 37 ,060	
		9′ 36″,9	mean of the six measures.

Sept. 30, 1791. *Correction of the clock* — 2′ 20″,6.

20ʰ 25′	1st measure	9′ 39″,258	
20 29	2d ——	9 38 ,441	
20 34	3d ——	9 37 ,596	
20 38	4th ——	9 37 ,032	
20 40	5th ——	9 40 ,949	
20 42	6th ——	9 37 ,793	
		9′ 38″,5	mean of the six measures.

Supposing the satellite now to be not far from its greatest elongation, I measured the declination between the centre of Saturn, and the 5th satellite; causing one to pass along one wire, while the other followed upon the other wire.

$22^h\ 32'$	1st	measure	$1'\ 41'',889$
22 47	2d	———	1 45 ,609
			$1'\ 43'',749$ mean of the two measures.

Not being satisfied with the considerable disagreement, I took another measure with the utmost precaution and care; as the apparent curvature of the wires at so great a distance, required more than common attention.

$0^h\ 52'$ very exact. $1'\ 43'',354$

Mean between this and the former mean, $1'\ 43'',55$ south of the parallel of Saturn.

The satellite not being perhaps arrived at its greatest elongation, I took six other measures of its distance.

$1^h\ 5'$	1st	measure	$9'\ 41'',907$
1 11	2d	———	9 38 ,723
1 14	3d	———	9 38 ,159
1 18	4th	———	9 41 ,203
1 23	5th	———	9 40 ,385
1 26	6th	———	9 41 ,935
			$9'\ 40'',4$ mean of the six measures.

In the last six measures of the 5th satellite, I used a method a very little different from that which I employed before, and which is probably more accurate. I used to observe, when the

two wires were nearly brought to their proper distance, the moment of intersection of the satellite; and the instant it was hid behind the wire, cast my eye on Saturn, which should be bisected when the measure is justly taken. But this change of attention cannot be made without some very small loss of time. To correct this defect I took alternately the bisection of Saturn, and cast my eye upon the satellite; and the bisection of the satellite, casting the eye upon Saturn. As the latter way gives the interval too small, the former gives it too large, and between both the true measure may be obtained. I do not, however, suppose, that the error of the former method can amount to so much as a single second of space; as, knowing the loss of time, I always used the utmost precaution; and repeated the examination of a measure perhaps 20 times before I let it pass.

Oct. 1, 1791. *Correction of the clock* — 2′ 20″,0.

22^h 25′	1st	measure	9′ 43″,767
22 27	2d	———	9 44 ,444
22 30	3d	———	9 43 ,007
22 32	4th	———	9 42 ,499
22 34	5th	———	9 40 ,554
22 36	6th	———	9 43 ,965

9′ 43″,0 mean of the six measures.

It grew cloudy, so that no measures later in the night could be obtained; nor could I get another sight of Saturn till October the 7th, when the satellite was far advanced in its orbit, on its return towards the planet.

Supposing the satellite to have been very nearly at its

greatest elongation, when the last six measures were taken, I have reduced them to the mean distance of Saturn, where they give 8′ 31″,97.

I forbear making deductions from this result, with respect to the quantity of matter contained in the planet, as, possibly, the orbit of the satellite may be considerably elliptical; in which case measures taken in opposite parts of that orbit will be required, before we can make a strict application of the laws of centripetal forces.

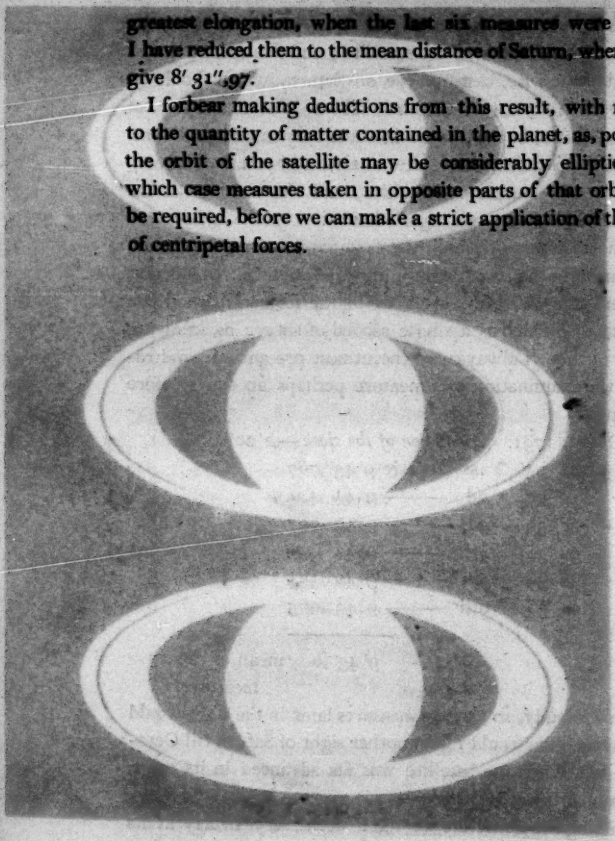


Fig. 1

Fig. 2

Fig. 3

II. *Miscellaneous Observations.* By William Herschel, *LL.D. F.R.S.*

Read December 22, 1791.

Account of a Comet.

LAST Thursday evening, the 15th of December, about half after eight o'clock, while I was taken up with observing Saturn, my sister looked over the heavens, and discovered a pretty large, telescopic comet, in the breast of Lacerta. I viewed it in my seven-feet reflector, and with that instrument settled its place and rate of moving. At $9^h\ 42'\ 4'',8$ true mean time, it preceded a small telescopic star $11'',3$ in time, and was $2'\ 41''$ south of the same. The place of this star I have since determined with sufficient accuracy, that it may be found again by those who wish to settle it more exactly. It follows the 2d of FLAMSTEED's stars in the constellation of Lacerta, $1'\ 41'',5$ in time; and is $45'\ 40'',8$ more south than the same. The apparent motion of the comet on Thursday evening was direct, and at the rate of about three minutes of time in right ascension, and a little more than two degrees in polar distance *per* day; from which we may suppose that we shall keep it some time in view. Last night I examined it with a twenty-feet reflector, and found it to consist of a great light, pretty regularly scattered

about a condensed small part of five or six seconds in diameter; which resembled a kind of nucleus, but had not the least appearance of a solid body. Beside the scattered, and gradually diminishing light, which reached nearly to a distance of three minutes every way beyond the bright centre, there was also a faintly extended, ill defined, pretty broad ray, of about 15 minutes in length, directed towards the north following part of the heaven, which might be called the tail of the comet.

Its place for the same night (Dec. 16th) was determined by a five-feet Newtonian *Sweeper*, carrying an equilateral triangle in the focus of the eye-glass, not so large but that the three intersections, made by the wires at the three angles, may be distinctly perceived. At $5^h\ 49'\ 40'',6$ it preceded the 6th Lacerta $4'\ 58'',5$ in time, and was $52'\ 14'',5$ more north than that star.

On the periodical Appearance of o Ceti.

The changeable star in the neck of the Whale, *o* Ceti, continues its variations as usual, but with some considerable irregularities of brightness.

In the year 1779, as we have seen *, it excelled *α* Arietis so far as almost to rival Aldebaran; and continued in that state a full month.

In 1780, its greatest brightness was only like that of *δ* Ceti.

In the year 1781, it did not come up to the brightness of *δ*.

In 1782, this star increased to the size of *β* Ceti, and continued bright for more than twenty days.

In 1783, it did not only vanish to the naked eye, as usual, but disappeared so completely, that I could not find it with a

* Phil. Trans. Vol. LXX. page 338.

telescope, which permitted not a star of the 10th magnitude to escape me. When it increased again, it did not amount to the brightness of δ.

In 1784, I saw it only of the 8th magnitude in a twenty-feet reflector, but as I did not continue to observe it regularly, it might possibly change as usual.

In 1789, it arrived to the brightness of α Piscium, or rather excelled it.

In 1790, the greatest brightness was almost equal to that of α Ceti.

In the present year, I have seen it only of the magnitude of γ Ceti nearly; or between γ and δ; but, as bad weather has occasioned many interruptions, it may possibly have been larger.

The period of 333 days, assigned by BOUILLAUD, does not agree with present observations compared to those of FABRICIUS made on the 13th of August, 1596, when this star was in its greatest lustre. M. CASSINI also found, that his observations, in the beginning of August, 1703, when the star was brightest, did not agree with the interval of 333 days; and therefore, supposing the star to have changed 117 times since the epoch of FABRICIUS, he gave it a period of 334 days. This will, however, not agree with the present time of the changes; and it appears now that M. CASSINI ought to have assumed 118 instead of 117 variations; which would have pointed out a period of 331 days, and some hours.

That this is, probably, very near the real time of the star's variation, will be seen when we admit it to have undergone 214 changes between the 13th of August, 1596, and the 21st of October, 1790; by which long interval we obtain the period of 331 days, 10 hours, 19 minutes. It will, indeed, be necessary,

in order to reconcile all observations, to admit of some occasional deviations in the appearance of the star, amounting almost to a month; but that this is no more than we may allow, is pretty evident from the variations I have taken notice of within the last 14 years; besides, a period of 334 days could not be admitted without totally giving up all regularity in the returning appearance of the star.

I have taken the epoch of the 21st of October, 1790, as one of the best ascertained, moderate appearances I have been able to obtain; and believe it to be more proper for settling the period, than that which might be deduced from a brilliant blaze of the star, such as took place in 1779, owing to causes that are not regular, and therefore may be apprehended to disturb the general order of the change.

On the Disappearance of the 55th Herculis.

Among the changes that happen in the sidereal heavens we enumerate the loss of stars; but, notwithstanding the real destruction of an heavenly body may not be impossible, we have some reasons to think that the disappearance of a star is probably owing to causes which are of the same nature with those that act upon periodical stars, when they occasion their temporary occultations. This subject, however, being of great extent and consequence, we shall not enter into it at present, but only relate a recent instance of the kind.

Two stars of the 5th magnitude, whose places we find inserted in all our best catalogues, were to be seen in the neck of Hercules. They are the 54th and 55th of FLAMSTEED'S, in that constellation. In the year 1781, the 10th of October, I

examined them both, and marked down their colour, *red*. The 11th of April, 1782, I looked at them again, and noted my having seen them distinctly, with a power of 460; and that they were single stars.

The 24th of last May, I missed one of the two, and examining the spot again the 25th, and many times afterwards, found that one of them was not to be seen. The situation of the stars is such that, not having fixed instruments, I could not well determine which of the two was the lost one. I therefore requested the favour of my much esteemed friend, the astronomer royal, to ascertain the remaining star; and it appears from Dr. MASKELYNE'S answer to my letter, that the 55th Herculis is the one which we have lost.

Remarkable Phænomena in an Eclipse of the Moon.

The 22d of October, 1790, when the moon was totally eclipsed, I viewed the disk of it with a twenty-feet reflector, carrying a magnifying power of 360. In several parts of it I perceived many bright, red, luminous points. Most of them were small and round. The brightness of the moon, notwithstanding the great defalcation of light occasioned by the eclipse, would not permit me to view it long enough to take the places of these points. They were, indeed, very numerous; as I suppose that I saw, at least, one hundred and fifty of them. Their light did not much exceed that of Mons Porphyrites HEVELII.

We know too little of the surface of the moon to venture at a surmise of the cause from whence the great brightness, similarity, and remarkable colour of these points could arise.

Slough, Dec. 17, 1791.

III. *Experiments and Observations on the Production of Light from different Bodies, by Heat and by Attrition.* By Mr. Thomas Wedgwood; *communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read December 22, 1791.

BEFORE I begin to state the experiments which are the subject of this Paper, it may not perhaps be improper to give a very compendious history of the discoveries which have already been made relative to phosphoric bodies; omitting, however, the electrical phosphori, and such as are evidently consumed or decomposed in the emission of their light, as these are well known, and are too numerous and important to be slightly noticed.

PLINY was well acquainted with the luminous appearance of rotten wood, and of the eyes of dead fish. From this time I find nothing relative to the phosphorism of bodies, till the beginning of the sixteenth century, when BENVENUTO CELLINI, in his Art of Jewellery, mentions his having seen a carbuncle shine in the dark like coals nearly burnt out; and relates a story of a coloured carbuncle having been found in a vineyard near Rome, by its shining in the night. About the year 1639, VINCENZO CASCARIOLO, of Bologna, discovered, by accident, that when a certain stone found in that neighbourhood was

calcined in a particular manner, it acquired the remarkable property of absorbing the light of the sun, of retaining it for some time, and of emitting it in the dark: subsequent experimenters found it to do the same with the light of a candle. In 1663, Mr. BOYLE observed a particular diamond to give out a light almost equal to that of a glow-worm, when heated, rubbed, or pressed; and investigated very fully the nature of the light of dead fish, flesh meat, and rotten wood. In 1677, BALDWIN of Misnia discovered, in the residuum of a distillation of chalk and nitrous acid, a phosphorus similar in its properties to the Bolognian, but not possessing the phosphoric virtue in so eminent a degree. In 1705, Mr. FRANCIS HAWKESBEE found that glass rubbed on glass, in common air, in the vacuum of an air-pump, or under water, "exhibited a considerable light." In 1724, M. DU FAY discovered that almost all substances which could be reduced to a calx by fire only, or after solution in the nitrous acid, absorbed and emitted light like the phosphorus of CASCARIOLO and of BALDWIN; and that some diamonds, emeralds, and many other precious stones emitted light in the dark, after being exposed to the rays of the sun. About the same time, BECCARIA of Turin found almost every body in nature to be luminous after a similar exposure: he added, too, this very important discovery; that an artificial phosphorus, exposed to the light in a coloured glass vial, emits, in the dark, rays of the identical colour of the vial. Mr. MARGRAAF, by an analysis of the Bolognian stone, shews that it contains vitriolic acid united to calcareous earth, and that all gypseous stones treated like the Bolognian, provided they are pure from iron, become phosphorescent. About the year 1764, Mr. CANTON made a phosphorus of sulphur and oyster-shells calcined together, and

distinguished himself by many curious experiments made with it; he found that his phosphorus might be made to shine by heating it, after it had ceased to be luminous of itself, but that the same heat would have this effect for a certain time only. Heat has been observed by several of these philosophers to promote the emission, and to shorten the duration, of the light of phosphori. Fluor has been long known to give a fine bright light when heated. D. HOFFMAN discovered that red blende and feldspat were luminous when pieces of either were rubbed together. PORT extended this discovery to all pure flints and crystals, and to porcelain. KEYSLER found glacies mariæ to be luminous when heated. M. DE LA METHERIE has observed some neutral salts and calcareous earths to be luminous in the same way. The Count de RAZOUMOWSKI, in a Memoir of the Physical Society of Lausanne, shews that quartz and glass give out light, when struck by almost any hard body, and that some few other bodies are luminous, when pieces of the same kind are rubbed upon one another; he finds quartz to give out its light under water.

This brief account includes, as far as I am able to collect, the chief discoveries which have been made concerning luminous bodies. I was led to make the following experiments from observing the light which proceeds from two quartz pebbles rubbed against each other: I searched for this property in many other bodies with success, but met with two soft stones, which did not afford any light upon the most violent attrition. Conceiving that heat might probably be the cause of the light emitted by quartz from attrition, I attributed this failure to a want of sufficient hardness in these friable stones for producing the necessary heat. Accordingly, sprinkling some

of their powder on a plate of iron nearly red hot, I had the satisfaction to observe it emitting a considerable light. Extending this mode of trial, I found that the phosphorism of almost all bodies might be made apparent either by heat or by attrition; I shall therefore divide the subject of this Paper into two parts: I. On the light produced by Heat.—II. On the light produced by Attrition.

I.

The best general method of producing the light by heat is, to reduce the body to a moderately fine powder, and to sprinkle it, by small portions at a time, on a thick plate of iron, or mass of burnt luting made of sand and clay, heated just below visible redness, and removed into a perfectly dark place.

The following is a list of such bodies as I have found to be luminous by this treatment, arranged according to the apparent intensity of their light.

1. Blue fluor, from Derbyshire, giving out a fetid smell on attrition.
2. Black and grey marbles, and fetid white marbles, from Derbyshire.
   Common blue fluor, from Derbyshire.
   Red feldspat, from Saxony.
3. Diamond.
   Oriental ruby.
   Aerated barytes, from Chorley, in Lancashire.
   Common whiting.
   Iceland spar.
   Sea shells.
   Moorstone, from Cornwall.

White fluor, from Derbyshire.

4. Pure calcareous earth, precipitated from an acid solution.
——argillaceous earth (of alum).
——siliceous earth.
——new earth, from Sydney Cove.
Common magnesia.
Vitriolated barytes, from Scotland.
Steatites, from Cornwall.
Alabaster.
Porcelain clay of Cornwall.
Mother of pearl.
Black flint.
Hard white marble.
Rock crystal, from the East Indies.
White quartz.
Porcelain.
Common earthen ware.
Whinstone.
Emery.
Coal ashes.
Sea sand.

5. Gold, platina, silver, copper, iron, lead, tin, bismuth, cobalt, zink:

Precipitates by an alkali from acid solutions of gold, silver, copper, iron, zink, bismuth, tin, lead, cobalt, mercury, antimony, manganese.

Vitriolated tartar,
Crystals of tartar,
Borax,
Alum,
} previously exsiccated.

Sea coal.
White paper,
——— linen, } in small pieces.
——— woollen,
——— hair powder.
Deal sawdust.
Rotten wood (not otherwise luminous).
White asbestos.
Red irony mica.
Deep red porcelain.
6. Antimony, nickel.
Oils, lamp, linseed, and olive,
White wax,
Spermaceti, } luminous at and below boiling.
Butter,

The duration of the light thus produced from different bodies is very unequal; in some the light is almost momentary, in others it lasts for some minutes, and may be prolonged by stirring the powder on the heater. It soon attains its greatest brightness, and dies away gradually from that point, never appearing in a sudden flash, like the light of quartz pebbles rubbed together. If blown upon, it is suddenly extinguished, but immediately re-appears on discontinuing the blast.

The light of bodies is, in general, uncoloured; there are, however, some exceptions. Blue fluor, of that kind which gives out a fetid smell when rubbed, first emits a bright green light, resembling that of the glow-worm so exactly, that when placed by the insect just as it has attained its greatest brightness, there is no sensible difference in the two lights, either of colour or intensity. This bright green quickly

changes into a beautiful lilac, which gradually fades away. Fetid marbles, and some kinds of chalk, give a bright reddish or orange light; pure calcareous earth, a bluish white light; Cornish moorstone emits a fine blue light; powder of ruby gives a beautiful red light, of short continuance.

The most phosphorescent marble is soft and friable, of a coarse crystallized grain, and a fetid odour when rubbed; black and grey marbles are generally more luminous than the white. Most of the common white marbles are hard, and of a fine grain, and they are not very luminous, nor is their light of an orange colour. Different chalks vary as much as different marbles, in the intensity and colour of their light, when no difference of external structure is perceptible. The most phosphorescent chalk loses the brilliancy and redness of its light by being dissolved in an acid, and precipitated by caustic fixed vegetable alkali—by being combined with vitriolic or fluor acid—by calcination by heat, or being combined with the aerial acid in the pellicle formed on the surface of lime-water. Marble would probably be affected in a similar manner. The most phosphorescent blue fluor gives the same light after being united to the vitriolic acid, though gypsum is far less luminous than fluor, and its light is colourless. Argil precipitated from alum by an alkali, and magnesia, when combined with fluor acid, give out the same light as before.

Bodies emit their light when immersed in boiling acid of vitriol, or in boiling oils; small lumps of fluor or of marble make a singular appearance in the acid, as they are moved up and down by its action, and rendered brightly luminous by the heat: they seem equally luminous in pure, fixed, inflammable, or atmospheric air.

Feldspat, the fetid fluor, and probably all phosphorescent

bodies, dropt, in moderately fine powder, into a flask containing a small quantity of boiling oil at the bottom, emit a copious flash of light as soon as the powder touches the surface of the oil; when the particles of the body have lain at the bottom of the heated fluid for about a minute, they become but faintly luminous; if the flask be then agitated so as to raise some of these particles out of the oil, and lodge them on its sides, they suddenly rekindle into the same brightness as at first, and preserve this re-assumed lustre for some time; and even after being again washed down into the oil, they may be readily distinguished from the particles which have remained at the bottom. This experiment is extremely beautiful, and is not at all obstructed by the faint light of the oil; it succeeds best with the stinking blue fluor of Derbyshire.

Powdered marble, and probably every other body, when spread upon the heater, in the receiver of an air-pump, is equally luminous during the exhaustion and re-admission of the air.

Bodies are by far most luminous the first time they are heated, but cannot, perhaps, be entirely deprived of this property by any number of heatings, nor by any degree of heat. Chalk, fluor, and feldspat, give out a very faint light on the heater, after having been exposed to a smart red heat in an open crucible, in small quantities, and kept frequently stirred for several hours; the feldspat was equally luminous when laid hot upon the heater, or first cooled, and then laid on. Chalk and fluor were not tried in this particular. A bit of glass, melted in a heat of 120° of my father's thermometer, and as soon as it is cold reduced to powder, gives out light on being thrown upon the heater below redness.

Quartz, from the same original piece, is equally luminous when the *powder* is directly thrown upon the heater—when it is previously made red hot, and then cooled and thrown on—or when a *fragment of some size* has been made red hot, then pounded and thrown on.

For the most part, the softest bodies require the least heat to become luminous; marble, chalk, fluor, &c. give a faint light when sprinkled on melted tin just becoming solid. As the temperature of the heater is raised, they continue to give out more and more light.

Vitriols of iron, copper, and zink, previously exsiccated, when thrown on earthen ware or metal made nearly red hot, give minute flashes of light of momentary duration, such as appear from some of the metallic precipitates, particularly zink, on a similar treatment; with this difference, however, that the light of most of the precipitates is of a reddish hue.

The light of the metals is white, and exactly similar to that of some earths.

White paper, when dipped in a solution of sal ammoniac, and slowly dried, becomes black upon the heater, and then gives out much less light than common paper.

If a lump, of the size of a small bean, of fluor, marble, feldspat, or any of the most phosphorescent bodies, be laid upon the heater, the light proceeds gradually upwards from the part in contact with the heater, till the whole mass is thoroughly illuminated; if the same piece be heated a second time, it is much less luminous; nor, if it be broken, are the fragments at all more luminous, either then, or after having been exposed for a month to the light and sunshine.

A little boiling oil at the bottom of a glass flask, when

agitated in the dark, illuminates the whole of the flask. The light of boiling oils proceeds, probably, from some kind of inflammation, as it is scarcely discernible unless the vessel be agitated; and, if a little oil be thinly spread on the heater, a subtle lambent flame, of a bluish hue, instantly arises. The same thing takes place if horn, bone, hair, saliva, or any animal matter be laid upon the heater.

II.

The experiments on the light produced from different bodies by attrition, were chiefly made by rubbing in the dark two pieces of the same kind against each other: all that I tried, with a very few exceptions, were luminous by this treatment. The following is a list of them, arranged in the order of the apparent intensity of their light, and as the lights are either white, or some shade of red, I have affixed figures to denote these differences; (0) denoting a pure white light; (1), the faintest tinge of red, or flame colour; (2), a deeper shade of red; (3) and (4), still deeper shades.

1. Colourless, transparent, oriental rock crystal; and siliceous crystals (0).
2. Diamond (0).
3. White quartz; white semitransparent agate (1).
4. White agate, more opake (2).
   Semitransparent feldspat, from Scotland (2).
   Brown opake feldspat, from Saxony (4).
   Chert of a dusky white, from North Wales (3).
5. Oriental ruby (4).
6. Topaz; oriental sapphire (0).
7. Agate, deep coloured, brown and opake (4).

8. Clear, blackish gun-flint (2).
9. Tawney semitransparent flint (3).
10. Unglazed white biscuit earthen ware (4).
11. Fine white porcelain (2).
12. Clear, blackish gun-flint, made opake by heat (3).
13. Flint glass (0).
14. Plate glass; green bottle glass (0).
15. Fine hard loaf sugar (0).
16. Moorstone, from Cornwall (1).
    Corune, semitransparent, from the East Indies (1).
17. Iceland spar (0).
18. White enamel (2); tobacco pipe (3).
    White Mica (0).
19. Unglazed biscuit earthen ware, blackened by exposing it, buried in charcoal in a close crucible, to a white heat (4.)
20. * Black vitreous mass, made by melting together 5 of fluor, 1 of lime, and some charcoal powder (4).
21. Fluor; aerated and vitriolated barytes; white and black Derbyshire marble; calcareous spar; crystals of borax; deep blue glass; mother of pearl.

Rock crystal, quartz, flint glass, and many other hard bodies, during attrition, emit now and then reddish sparks of a vivid light, which retain their brightness in a passage of one, two, and even three inches, through the air.

A piece of opake agate, applied to the circumference of a wheel of fine grit, revolving at a moderate rate, becomes

* Some of this mixture taken out of the crucible before it was perfectly fused, gave out, when rubbed, a strong smell like phosphorus of urine; and on throwing some of it pulverized on a plate of iron, heated just below redness, it was very luminous, and presented every appearance of burning phosphorus.

brightly red, even in day-light, at the touching part; if the wheel revolve at a quicker rate, the touching part emits a pure white light. In both cases, glowing sparks are continually emitted, some of which are not extinguished before they have passed twelve or fourteen inches through the air; they explode gunpowder and inflammable air, and burn the skin; their brightness is not sensibly increased by passing into pure air. The corner of an angular piece of window glass being applied to the wheel in motion, a full eighth of an inch of the glass above the point of contact becomes, apparently, red hot, and retains the redness for a second or two of time after its removal from the wheel; during the attrition, large red sparks are continually emitted, and a mixture of softened glass, and the sand of the stone wheel, is collected about the touching point. Quartz, transparent agate, rock crystal, and window glass, give nearly the same flashing light, when rubbed against the stone wheel, or in the ordinary manner, excepting the tinge of red in the former, which it receives from the light of the grit: the transparent agate becomes red hot for a little way about the part in contact with the wheel, and is thus deprived of its transparency, as it would be if made red hot in a common fire; porcelain is heated to redness by the same treatment. The red sparks which are emitted by all these bodies during their attrition, are heated particles about the magnitude of grains of fine sand, broken off by the friction.

Bodies give out their light the instant they are rubbed upon each other, and cease to be luminous when the attrition is discontinued. Colourless, transparent, and semitransparent bodies emit a flashing light, their whole masses being, for a moment, illuminated; opake bodies give little more than a

defined speck of red light, and are not luminous below the part struck. The greatest apparent quantity of light is produced by hard, uncoloured, transparent, and semitransparent bodies, whose surfaces soon acquire an asperity by rubbing together, as quartz, agate, &c. From an examination of the table, it appears that white lights are emitted from colourless transparent bodies; faint red, or flame-coloured, from white semitransparent bodies; deeper red from more opake and coloured bodies, and the deepest red from opake and from deep-coloured bodies. Extremely faint lights, such as those given by fluor, marble, &c. are of a bluish white; quartz, very lightly rubbed, gives a very faint light of a bluish hue; when rubbed a little harder, it emits a flame-coloured light; when rubbed with violence, its light approaches to whiteness. Opake red feldspat gives a deep red light by attrition; exposed to a strong heat in the furnace, it becomes white, and somewhat transparent, and when cool, gives out, on attrition, as white a light as quartz; clear, blackish flint, made opake by heat, gives a redder light than before; deep-coloured glass gives out a red defined light without any flash, whilst clear uncoloured glasses emit a white flashing light of some brightness.

Bodies are not luminous by simple pressure; but when they are at all broken by the pressure, the fragments rubbing on each other produce some light. Mr. BOYLE, indeed, found a particular diamond to emit light when pressed by a steel bodkin; but the diamond is phosphorescent in so many ways, and is so curious and singular a body, both in properties and constitution, that it can scarcely be expected to exhibit the same appearances as the common class of earthy bodies.

Alum, indurated by having been kept long in a state of

fusion, and being then much harder than loaf sugar or borax, both which are luminous from moderate attrition, gives no light, though rubbed with much violence*.

If two pieces of glass or quartz be strongly rubbed against each other, and then applied to the fine down of a feather, the down is not sensibly affected; if the same glass be rubbed on woollen cloth, and placed near the feather, the down is immediately attracted.

Rock crystal, quartz, feldspat, white unglazed earthen ware, Derbyshire black marble, and probably all phosphorescent bodies, insoluble in water, give out their light on rubbing them under water, as copiously as in air. Hard white sugar, from the outside of the loaf, gives out its light when rubbed in oil. Bodies seem equally luminous in atmospheric, pure, fixed, and inflammable, air.

All hard earthy bodies emit a peculiar smell on attrition. The most remarkable for this property are chert, quartz, feldspat, biscuit earthen ware, and rock crystal: this smell does not differ much in kind, though it does considerably in intensity. Many of the softer bodies yield the same smell, but in a less degree, and, probably, none are entirely without it. It appears to be strongest where the friction is greatest: it

* The Count DE RAZOUMOWSKI has investigated the luminous property of bodies in a way which appears to me very unfavourable for the discovery of their true lights. He rubbed, not one piece against another of the same body, but, all of them against quartz or glass: he finds several metals luminous from this treatment, and attempts to draw some curious conclusions from the colour of their lights. I tried these metals in his own way, and found that no light was emitted except when the violence of the blow shattered the quartz or glass; a piece of the indurated alum will excite light from rock crystal, by breaking its surface, but this is the light of the fragments of the crystal rubbing on each other, and not of the alum.

has no dependence on the light produced by attrition, as it is often very strong when no light is emitted.

Rock crystal, quartz, feldspat, white biscuit earthen ware, and probably all such hard bodies, produce this smell under water.

Quartz stones, violently rubbed upon one another for a few minutes in a cup of water, communicate this smell, and a peculiar taste, to the water. The taste is probably derived from an impalpable powder, which floats in the water for many days.

Derbyshire black marble, and the stinking blue fluor, give out, on attrition, a strong smell peculiar to themselves, both in air and water; they lose this property by being once made red hot.

Quartz produces the smell equally strong in fixed, pure, and common, air.

Having now stated all the facts relative to phosphorescent bodies which I have as yet been able to discover, I shall beg leave to offer a few reflections, tending to shew, that heat is the probable cause of the light produced from bodies by attrition.

The powders of all earthy bodies emit light when heated a little under redness. Now, when two bodies are rubbed upon each other, it is probable that heat is *always* generated on their surfaces: may not then the light which they yield upon attrition be attributed to a sudden heating of particles in their surfaces? for these particles will be affected in the same way as if they had been equally heated by any other means; they will therefore give the same light as if they had been laid upon the heater of an equal temperature.

The shining sparks which hard bodies send out during

attrition, prove, that particles in their surfaces are heated at least to more than 600° of Fahrenheit, for the powders of hard bodies are not luminous on a heater much below redness. The heat generated by the attrition of soft friable bodies is probably but little, as the minute particles in their surfaces are not much crushed by each other, but merely disjoined from the masses; nor can this little be easily appreciated, for as the surfaces continually crumble away, the heated parts are carried off before they can warm the masses, and are themselves very quickly cooled by the surrounding air. But as many soft bodies emit a faint light on the heater at the low temperature of about 400° of Fahrenheit, and as it is not a stronger light which they yield on attrition, little heat is required to render the particles in their surfaces luminous. It must be observed too, that, though the absolute quantity of heat generated by one rub be but inconsiderable, the effects of it may be very striking; for just on the instant of attrition, its action is confined to the minute colliding points of the surfaces, and will consequently operate upon them as much as a greater quantity would on larger points.

The light emitted by bodies in attrition is of momentary duration, whereas a powder on the heater continues to emit light for some minutes: this difference is easily explained; in the latter case, the particles are constantly heated; in the former, they are instantly cooled by the subjacent mass to a temperature in which bodies are not luminous, *i. e.* a little under 400° of Fahrenheit.

When the uneven surfaces of bodies are rubbed upon each other, a flashing light is produced at frequent intervals, by the

collision of the more prominent parts; this, in colourless, transparent, and semitransparent bodies, is copiously reflected from the whole of the masses, and forms an appearance very different from what is ever exhibited by bodies on the heater: it may be well illustrated in the upper part of a candle, by repeatedly nearly closing, and quickly opening, the snuffers, about the burning wick; or by sprinkling some powder of fluor or marble on a mass of glass heated just under redness.

Powder of crystal, quartz, agate, &c. is but faintly luminous on the heater under redness; accordingly, if the stones themselves be gently rubbed, a faint light is emitted, resembling that which their powders give out on the heater. Marble and fluor give about the same light on the heater of the temperature of 400°, as they do when rubbed; and probably attrition heats some particles in their surfaces to that degree.

It may, at first, seem an objection to the opinion of heat being the cause of the light produced from bodies by attrition, that they yield their light when rubbed under water; but the water acts differently to the air, only, as being a stronger conductor of heat, and can nowise impede the actual generation of the heat; now, as bodies emit their light on the very instant of their being heated, the water cannot cool the hot particles before they have given their light.

It is easy to see why bodies emit light *instantly* when rubbed; for they often send out sparks as soon as the attrition commences, which proves that particles in their surfaces are *instantly* heated to redness by attrition.

Since hard bodies may be heated to redness by attrition, we have an excellent method of discovering the lights they give out

at that temperature, which could not be effected by sprinkling their powders on a red hot heater, as the light of the powder would be mixed with that of the heater. In some cases of attrition, bodies are raised to a temperature beyond visible red heat. The corner of an angular piece of window glass being applied to the circumference of a revolving wheel of fine grit, part of its mass is worn away; but a larger portion, lying just above the abraded part, is heated to redness. Now, as all the heat which is there collected, and a great deal more, which is carried away in the abraded part, and conducted off by the air, and by the glass lying up to the red hot portion, has once occupied a smaller space in the part worn away; it follows, that the abraded portion, or aggregate of heated surfaces, has been heated to a degree exceeding redness, by all the heat remaining in the red hot part, and by the quantity of heat conducted off by the air and the adjacent glass; and, consequently, that each surface has been heated by the attrition to a degree *as* much exceeding redness.

I am aware that this reasoning is founded, in part, on the supposition that the heat is generated on the surface or outermost coat of the body: some of it may undoubtedly proceed from an agitation of parts under the surface; but the emission of red hot sparks at the *instant* of attrition, proves that a great heat is generated on the surface; and, as the friction, or apparent heating cause, is so much greater there, that the parts are forcibly broken and disjoined, whilst just beneath there is no perceptible alteration in the body, we may venture to conclude that the heat generated beneath the surface is but inconsiderable.

After all, it remains entirely problematical, in what manner heat operates to produce light from bodies: the air does not seem to have any concern in its production, as bodies are equally luminous in almost all kinds of air, and when immersed in liquids. The phosphorism of sugar is probably of a different kind from that of the earthy class; for, though so soft and friable a substance, it produces its light very copiously upon gentle attrition.

In speaking of the attrition of bodies upon the stone wheel, I have said that they became *red hot* about the touching part; I should not have made use of this expression if the luminous sparks, which issued from them, had not kindled gunpowder and inflammable air, and thus proved that the part from which they came was raised to a temperature, at least equal to what is usually termed a *red heat*; for till the temperature of the part touching the wheel had been thus ascertained, I attributed the whole of the light emitted to the common phosphorism of bodies. If the velocity of the wheel be much increased, the touching part of the body applied, emits a bright white light, much more vivid than any which powders ever give out on the heater, and, probably, the temperature of the luminous part is equal to what is usually called a *white heat*.

Having thus made incombustible bodies red hot without the aid of fire, I once conceived that all the light which they emit when heated to redness in the fire, proceeded entirely from their great phosphorism; for I could not suppose that they absorbed light from the burning fuel and emitted it again, at the same time, and during a continuance of the same circumstances. It appeared, however, equally inexplicable, why a stone put into

the fire, should continue to shine from its own light, with undiminished lustre, as long as the fire is kept up; for it has been shewn, that if a phosphorescent body remain long upon the heater, of any temperature between 400° of Fahrenheit and a red heat, its light diminishes more and more, till at last it is scarcely perceptible; and then an increase of heat is necessary to render it more luminous.

APPENDIX.

After a considerable part of the above paper was printed, I repeated the experiment with boiling oil, related in p. 35, with every possible precaution. I poured the powder into the flask through a funnel which reached to the bottom, so that none of it might be lodged on the sides; for, not having attended to this circumstance before, I was apprehensive that the experiment was delusive, and that the agitation of the hot oil might have washed down some fresh particles which had not been before heated;—then slowly pouring in a little oil, I boiled it for a few minutes, and removed it into a dark place. When the powder had become but faintly luminous, upon agitation, the experiment succeeded exactly. I then boiled and agitated the oil for six or seven times successively, with the same result, except that the light of the powder grew something fainter each time. If any one has the curiosity to repeat this singular experiment, he may attend to the following directions.—Upon each removal from the fire, stop the neck of the flask with a cork, having a small hole pierced through it—wrap the neck round with tow—agitate the liquor by quickly raising and lowering the flask.

The sparks which leave the surfaces of hard bodies during attrition, may be exactly imitated by burning a bit of dusty paper;—the particles of dust are carried by the current of air through the blaze, and are made red hot in their passage.

IV. *Experiments upon Heat. By Major-General Sir* Benjamin Thompson, *Knt. F.R.S. In a Letter to Sir* Joseph Banks, *Bart. P.R.S.*

Read January 19, 1792.

DEAR SIR, *Munich, June*, 1787.

SINCE my last communication upon the subject of HEAT, which the Royal Society have done me the honour to publish in their Transactions, I have made some further progress in the investigation of that most interesting subject, of which I propose to give you an account in this letter.

The confining and directing of heat are objects of such vast importance in the œconomy of human life, that I have been induced to confine my researches chiefly to those points, conceiving that very great advantages to mankind could not fail to be derived from the discovery of any new facts relative to these operations.

If the laws of the communication of heat from one body to another were known, measures might be taken with certainty, in all cases, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewise greatly increase the comforts and conveniencies of life; objects of which the philosopher should never lose sight.

The route which I have followed in this inquiry is that which I thought bid fairest to lead to useful discoveries. Without embarrassing myself with any particular theory, I have formed to myself a plan of experimental investigation, which I conceived would conduct me to the knowledge of certain facts, of which we are now ignorant, or very imperfectly informed, and with which it is of consequence that we should be made acquainted.

The first great object which I had in view in this inquiry was to ascertain, if possible, the cause of the warmth of certain bodies; or the circumstances upon which their power of confining heat depends. This, in other words, is no other than to determine the cause of the conducting and non-conducting power of bodies.

To this end I began by determining by actual experiment the relative conducting powers of various bodies of very different natures, both fluids and solids, of some of which experiments I have already given an account in the Paper above mentioned, which is published in the Transactions of the Royal Society for the year 1786; I shall now, taking up the matter where I left it, give you the continuation of the history of my researches.

Having discovered that the Torricellian vacuum is a much worse conductor of heat than common air, and having ascertained the relative conducting powers of air, of water, and of mercury, under different circumstances, I proceeded to examine the conducting powers of various solid bodies, and particularly of such substances as are commonly made use of for clothing.

The method of making these experiments was as follows: a mercurial thermometer*, whose bulb was about $\frac{11}{100}$ of an inch

* See Phil. Trans. Vol. LXXVI. Tab. VI. Fig. 4.

in diameter, and its tube, about 10 inches in length, was suspended in the axis of a cylindrical glass tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $1\frac{1}{10}$ inch in diameter, in such a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the space between the internal surface of the globe and the surface of the bulb of the thermometer being filled with the substance whose conducting power was to be determined, the instrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were observed, and noted down.

The tube of the thermometer was divided at every tenth degree from 0°, or the point of freezing, to 80°, that of boiling water, and these divisions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was seen through it.

The thermometer was confined in its place by means of a stopple of cork, about $1\frac{1}{2}$ inch long, fitted to the mouth of the cylindrical tube, through the centre of which the end of the tube of the thermometer passed, and in which it was cemented.

The operation of introducing into the globe the substances whose conducting powers are to be determined, is performed in the following manner; the thermometer being taken out of the cylindrical tube, about two-thirds of the substance which is to be the subject of the experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the substance being placed round about the tube of the thermo-

meter; and, lastly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the substance which, being introduced last, remains in the cylindrical tube above the bulb of the thermometer, is pushed down into the globe, and placed equally round the bulb of the thermometer by means of a brass wire which is passed through holes made for that purpose in the stopple closing the end of the cylindrical tube.

As this instrument is calculated merely for measuring the passage of heat in the substance whose conducting power is examined, I shall give it the name of *passage-thermometer*, and I shall apply the same appellation to all other instruments constructed upon the same principles, and for the same use, which I may in future have occasion to mention; and as this instrument has been so particularly described, both here, and in my former Paper upon the subject of heat, in speaking of any others of the same kind in future it will not be necessary to enter into such minute details. I shall, therefore, only mention their *sizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and divisions of their tubes, taking it for granted that this will be quite sufficient to give a clear idea of the instrument.

In most of my former experiments, in order to ascertain the conducting power of any body, the body being introduced into the globe of the passage-thermometer, the instrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged suddenly into boiling water, and the times of heating from ten to ten degrees were observed and noted; and I said that these times were as the conducting power of the body inversely; but in the experi-

ments of which I am now about to give an account, I have in general reversed the operation; that is to say, instead of observing the times of heating, I have first heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taken up in cooling.

I have preferred this last method to the former, not only on account of the greater ease and convenience with which a thermometer, plunged into a mixture of water, may be observed, than when placed in a vessel of boiling water, and surrounded by hot steam, but also on account of the greater accuracy of the experiment, the heat of boiling water varying with the variations of the pressure of the atmosphere: consequently the experiments made upon different days will have different results, and of course, strictly speaking, cannot be compared together; but the temperature of pounded ice and water is ever the same, and of course the results of the experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, as this temperature, as I have just observed, is variable; but when the mercury had attained the 75° of its scale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be still more accurate, suffering the mercury to rise a degree or two above 75°, and then taking it out of the boiling water, I held it over the vessel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, descending, passes the 75°.

Having a watch at my ear which beat half seconds (which I counted), I noted the time of the passage of the mercury

over the divisions of the thermometer, marking 70° and every tenth degree from it, descending, to 10° of the scale. I continued the cooling to 0°, or the temperature of the ice and water, in very few instances, as this took up much time, and was attended with no particular advantage, the determination of the times taken up in cooling 60 degrees of REAUMUR's scale, that is to say, from 70° to 10°, being quite sufficient to ascertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was constantly moved about in this mixture from one place to another; and there was always so much pounded ice mixed with the water, that the ice appeared above the surface of the water; the vessel, which was a large earthen jar, being first quite filled with pounded ice, and the water being afterwards poured upon it; and fresh quantities of pounded ice being added as the occasion required.

Having described the apparatus made use of in these experiments, and the manner of performing the different operations, I shall now proceed to give an account of the experiments themselves.

My first attempt was to discover the relative conducting powers of such substances as are commonly made use of for clothing; accordingly, having procured a quantity of *raw silk*, as spun by the worm, *sheep's wool*, *cotton wool*, *linen* in the form of the finest lint, being the scrapings of very fine Irish linen, the finest part of the *fur of the beaver*, separated from the skin, and from the long hair, the finest part of the *fur of a white Russian hare*, and *Eider down*; I introduced successively 16 grains in weight of each of these substances into the globe of the passage-thermometer, and placing it carefully

and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before described, and taking it out of the boiling water, plunged it into pounded ice and water, and observed the times of cooling.

But as the interstices of these bodies thus placed in the globe were filled with air, I first made the experiment with air alone, and took the result of that experiment, as a standard by which to compare all the others; the results of three experiments with air were as follows:

The bulb of the thermometer surrounded by air.

Heat lost.	Exp. No. 1. Time elapsed.	Exp. No. 2. Time elapsed.	Heat acquired.	Exp. No. 3. Time elapsed.
70°	—	—	10°	—
60°	38″	38″	20°	39″
50°	46	46	30°	48
40°	59	59	40°	58
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

The following table shows the results of the experiments, with the various substances therein mentioned:

Heat lost.	Air.	Raw silk. 16 grs.	Sheep's wool. 16 grs.	Cotton wool. 16 grs.	Fine lint. 16 grs.	Beavers fur. 16 grs.	Hares fur. 16 grs.	Eider down. 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Exp. 10.
70°	—	—	—	—	—	—	—	—
60°	38″	94″	79″	83″	80″	99″	97″	98″
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times.	576	1284	1118	1046	1032	1296	1315	1305

Now the *warmth* of a body, or its power to confine heat, being as its power of resisting the passage of heat through it (which I shall call its *non-conducting power*), and the time taken up by any body in cooling, which is surrounded by any medium through which the heat is obliged to pass, being, *cæteris paribus*, as the resistance which the medium opposes to the passage of the heat, it appears that the *warmth* of the bodies mentioned in the foregoing table are as the times of cooling; the *conducting powers* being inversely as those times, as I have formerly shown.

From the results of the foregoing experiments it appears, that of the seven different substances made use of, hares fur and Eider down were the warmest; after these came beavers fur; raw silk; sheep's wool; cotton wool; and lastly, lint, or the scrapings of fine linen; but I acknowledge that the

differences in the warmth of these substances were much less than I expected to have found them.

Suspecting that this might arise from the volumes or solid contents of the substances being different (though their weights were the same), arising from the difference of their specific gravities; and as it was not easy to determine the specific gravities of these substances with accuracy, in order to see how far any known difference in the volume or quantity of the same substance, confined always in the same space, would add to, or diminish, the time of cooling, or the apparent warmth of the covering, I made the three following experiments.

In the first, the bulb of the thermometer was surrounded by 16 grains of Eider down; in the second by 32 grains; and in the third by 64 grains; and in all these experiments the substance was made to occupy exactly the same space, viz. the whole internal capacity of the glass globe, in the centre of which the bulb of the thermometer was placed; consequently the thickness of the covering of the thermometer remained the same, while its density was varied in proportion to the numbers 1, 2, and 4.

The results of these experiments were as follow:

The bulb of the thermometer being surrounded by Eider down.			
Heat lost.	16 grains.	32 grains.	64 grains.
	(Exp. No. 11.)	(Exp. No. 12.)	(Exp. No. 13.)
70°	—	—	—
60°	97″	111″	112″
50°	117	128	130
40°	145	157	165
30°	192	207	224
20°	267	304	326
10°	486	565	658
Total times	1304	1472	1615

Without stopping at present to draw any particular conclusions from the results of these experiments, I shall proceed to give an account of some others, which will afford us a little further insight into the nature of some of the circumstances upon which the warmth of covering depends.

Finding, by the last experiments, that the density of the covering added so considerably to the warmth of it, its thickness remaining the same, I was now desirous of discovering how far the internal structure of it contributed to render it more or less pervious to heat, its thickness and quantity of matter remaining the same. By internal structure, I mean the disposition of the parts of the substance which forms the covering; thus they may be extremely divided, or very fine, as raw silk as spun by the worms, and they may be equally distributed through the whole space they occupy; or they may be coarser,

or in larger masses, with larger interstices, as the ravelings of cloth, or cuttings of threads.

If heat passed *through* the substances made use of for covering, and if the warmth of the covering depended solely upon the difficulty which the heat meets with in its passage through the substances, *or solid parts*, of which they are composed; in that case, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of which it is composed; but that this is not the case, the following, as well as the foregoing experiments clearly evince.

Having, in the experiment N° 4, ascertained the warmth of 16 grains of raw silk, I now repeated the experiment with the same quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common sewing silk, cut into lengths of about two inches.

The following table shows the results of these three experiments:

Heat lost.	Raw silk, 16 grs.	Ravelings of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60°	94″	90″	67″
50°	110	106	79
40°	133	128	99
30°	185	172	135
20°	273	246	195
10°	489	427	342
Total times.	1284	1169	917

Here, notwithstanding that the quantities of the silk were the same in the three experiments, and though in each of them it was made to occupy the same space, yet the warmth of the coverings which were formed were very different, owing to the different disposition of the material.

The raw silk was very fine, and was very equally distributed through the space it occupied, and it formed a warm covering.

The ravelings of taffety were also fine, but not so fine as the raw silk, and of course the interstices between its threads were greater, and it was less warm; but the cuttings of sewing silk were very coarse, and consequently it was very unequally distributed in the space in which it was confined; and it made a very bad covering for confining heat.

It is clear from the results of the five last experiments, that the air which occupies the interstices of bodies, made use of for covering, acts a very important part in the operation of confining heat; yet I shall postpone the examination of that circumstance till I shall have given an account of several other experiments, which, I think, will throw still more light upon that subject.

But, before I go any farther, I will give an account of three experiments which I made, or rather the same experiment which I repeated three times the same day, in order to see how far experiments made according to the method here described, may be depended on, as being regular in their results.

The glass globe of the passage-thermometer being filled with 16 grains of cotton-wool, the instrument was heated and cooled three times successively, when the times of cooling were observed as follows:

Heat lost.	Exp. 16.	Exp. 17.	Exp. 18.
70°	—	—	—
60°	82″	84″	83″
50°	96	95	95
40°	118	117	116
30°	152	153	151
20°	221	221	220
10°	380	377	377
Total times.	1049	1047	1042

The difference of the times of cooling in these three experiments were extremely small; but, regular as these experiments appear to have been in their results, they were not more so than the other experiments made in the same way, many of which were repeated two or three times, though, for the sake of brevity, I have put them down as single experiments.

But to proceed in the account of my investigations relative to the causes of the warmth of warm clothing. Having found that the fineness and equal distribution of a body or substance made use of to form a covering to confine heat, contributes so much to the warmth of the covering, I was desirous, in the next place to see the effect of condensing the covering, its quantity of matter remaining the same, but its thickness being diminished in proportion to the increase of its density.

The experiment I made for this purpose was as follows:— I took 16 grains of common sewing silk, neither very fine nor very coarse, and winding it about the bulb of the thermometer in such a manner that it entirely covered it, and was as nearly as possible of the same thickness in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling

water, cooled it in ice and water, as in the foregoing experiments. The results of the experiment were as may be seen in the following table; and in order that it may be compared with those made with the same quantity of silk differently disposed of, I have placed those experiments by the side of it:

Heat lost.	Raw silk, 16 grs.	Fine ravelings of taffety, 16 grs.	Sewing silk cut into lengths, 16 grs.	Sewing silk, 16 grs. wound round the bulb of the thermometer.
	Exp. No. 4.	Exp. No. 14.	Exp. No. 15	Exp. No. 19.
70°	—	—	—	—
60°	94″	90″	67″	46″
50°	110	106	79	62
40°	133	128	99	85
30°	185	172	135	121
20°	273	246	195	191
10°	489	427	342	399
Total times.	1284	1169	917	904

It is not a little remarkable, that, though the covering formed of sewing silk wound round the bulb of the thermometer in the 19th experiment, appeared to have so little power of confining the heat when the instrument was very hot, or when it was first plunged into the ice and water, yet afterwards, when the heat of the thermometer approached much nearer to that of the surrounding medium, its power of confining the heat which remained in the bulb of the thermometer appeared to be even greater than that of the silk in the experiment N° 15, the time of cooling from 20° to 10° being in the one 399″, and

in the other 342". The same appearance was observed in the following experiments, in which the bulb of the thermometer was surrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner as the sewing silk was wound round it in the last experiment.

The following table shows the results of these experiments, with the threads of various kinds; and that they may the more easily be compared with those made with the same quantity of the same substances in a different form, I have placed the accounts of these experiments by the side of each other. I have also added the account of an experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going round it nine times, and being bound together at the top and bottom of it, so as completely to cover it.

Heat lost.	*Sheep's wool*, 16 grains, surrounding the bulb of the thermometer.	*Woollen thread*, 16 grains, wound round the bulb of the thermometer.	*Cotton wool*, 16 grains, surrounding the bulb of the thermometer.	*Cotton thread*, 16 grains, wound round the bulb of the thermometer.	*Lint*, 16 grains, surrounding the bulb of the thermometer.	*Linen thread*, 16 grains, wound round the bulb of the thermometer.	*Linen cloth*, 16 grains, wrapped round the bulb of the thermometer.
	Exp. 5.	Exp. 20.	Exp. 6.	Exp. 21.	Exp. 7.	Exp. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60°	79"	46"	89"	45"	80"	46"	42"
50°	95	63	95	60	93	62	56
40°	118	89	117	83	115	83	74
30°	162	126	152	115	150	117	108
20°	238	200	221	179	218	180	168
10°	426	410	378	370	376	385	338
Tot. times.	1118	934	1046	852	1032	873	783

That thread wound light round the bulb of the thermometer should form a covering less warm than the same quantity of wool, or other raw materials of which the thread is made, surrounding the bulb of the thermometer in a more loose manner, and consequently occupying a greater space, is no more than what I expected, from the idea I had formed of the causes of the warmth of covering; but I confess I was much surprised to find that there is so great a difference in the relative warmth of these two coverings, when they are employed to confine great degrees of heat, and when the heat they confine is much less in proportion to the temperature of the surrounding medium. This difference was very remarkable; in the experiments with sheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60°, as 79 to 46 (the surrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the experiments with lint, and with linen thread, when the heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the experiments, when the heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it should seem that a covering may, under certain circumstances, be very good for confining small degrees of warmth, which would be but very indifferent when made use of for confining a more intense heat, and *vice versa*. This, I believe, is a new fact; and, I think the knowledge of it may

lead to further discoveries relative to the causes of the warmth of coverings, or the manner in which heat makes its passage through them. But I forbear to enlarge upon this subject, till I shall have given an account of several other experiments, which I think throw more light upon it, and which will consequently render the investigation easier and more satisfactory.

With a view to determine how far the power which certain bodies appear to possess of confining heat, when made use of as covering, depends upon the natures of those bodies, considered as chymical substances, or upon the chymical principles of which they are composed, I made the following experiments.

As charcoal is supposed to be composed almost entirely of phlogiston, I thought that, if that principle was the cause either of the conducting power, or the non-conducting power of the bodies which contain it, I should discover it by making the experiment with charcoal, as I had done with various other bodies. Accordingly, having filled the globe of the passage-thermometer with 176 grains of that substance in very fine powder (it having been pounded in a mortar, and sifted through a fine sieve), the bulb of the thermometer being surrounded by this powder, the instrument was heated in boiling water, and being afterwards plunged into a mixture of pounded ice and water, the times of cooling were observed as mentioned in the following table. I afterwards repeated the experiment with lampblack, and with very pure, and very dry wood ashes; the results of which experiments were as under mentioned:

Heat lost.	The bulb of the thermometer surrounded by			
	176 grains of fine powder of charcoal.	176 grains of fine powder of charcoal.	195 grains of lampblack.	307 grains of pure dry wood ashes.
	Exp. No. 24.	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60°	79″	91″	124″	96″
50°	95	91	118	92
40°	100	109	134	107
30°	139	133	164	136
20°	196	192	237	185
10°	331	321	394	311
Total times.	940	937	1171	927

The experiment, N° 25, was simply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former experiment, the powder of charcoal which filled the globe was shaken a little together, and to this circumstance I attribute the difference in the results of the two experiments.

In the experiments with lampblack and with wood ashes, the times taken up in cooling from 70° to 60° were greater than those employed in cooling from 60° to 50°; this most probably arose from the considerable quantity of heat contained by these substances, which was first to be disposed of, before they could receive and communicate to the surrounding medium that which was contained by the bulb of the thermometer.

The next experiment I made was with *semen lycopodii*, commonly called witch-meal, a substance which possesses very extraordinary properties. It is almost impossible to wet it; a quantity of it strewed upon the surface of a basin of water, not only swims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; so that a piece of money, or other solid body, may be taken from the bottom of the basin by the naked hand, without wetting the hand; which is one of the tricks commonly shown by the jugglers in the country: this meal covers the hand, and descending along with it to the bottom of the basin, defends it from the water. This substance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; so much so, that being blown out of a quill into the flame of a candle, it flashes like gunpowder, and it is made use of in this manner in our theatres for imitating lightning.

Conceiving that there must have been a strong attraction between this substance and air, and suspecting, from some circumstances attending some of the foregoing experiments, that the warmth of a covering depends not merely upon the fineness of the substance of which the covering is formed, and the disposition of its parts, but that it arises in some measure from a certain attraction between the substance and the air which fills its interstices, I thought that an experiment with *semen lycopodii* might possibly throw some light upon this matter; and in this opinion I was not altogether mistaken, as will appear by the results of the three following experiments.

The bulb of the thermometer surrounded by 256 grains of *semen lycopodii*.				
Heat lost.	Cooled. Exp. No. 28.	Cooled. Exp. No. 29.	Heat acquired.	Heated. Exp. No. 30.
70°	—	—	0°	—
60°	146″	157″	10°	230″
50°	162	160	20°	68
40°	175	170	30°	68
30°	209	203	40°	76
20°	284	288	50°	121
10°	502	513	60°	316
—	—	—	70°	1585
Total times.	1478	1491	—	2459

In the last experiment (N° 30) the result of which was so very extraordinary, the instrument was cooled to 0° in thawing ice, after which it was plunged suddenly into boiling water, where it remained till the inclosed thermometer had acquired the heat of 70°, which took up no less than 2456 seconds, or above 40 minutes; and it had remained in the boiling water full a minute and an half before the mercury in the thermometer shewed the least sign of rising. Having at length been put into motion, it rose very rapidly 40 or 50 degrees, after which its motion gradually abating became so slow, that it took up 1585 seconds, or something more than 26 minutes, in rising from 60° to 70°, though the temperature of the medium in which it was placed during the whole of this time was very nearly 80°; the mercury in the barometer standing but little short of 27 Paris inches.

All the different substances which I had yet made use of in these experiments for surrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a less degree, confined the heat, or prevented its passing into or out of the thermometer so rapidly as it would have done, had there been nothing but air in the glass globe, in the centre of which the bulb of the thermometer was suspended. But the great question is, how, or in what manner, they produced this effect?

And first, it was not in consequence of their own non-conducting powers, simply considered; for, if instead of being only bad conductors of heat, we suppose them to have been totally impervious to heat, their volumes or solid contents were so exceedingly small in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interstices, that air would have been sufficient to have conducted all the heat communicated, in less time than was actually taken up in the experiment.

The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch) the space between the bulb of the thermometer and the internal surface of the globe amounted to 2,1446−60,08711=2,05755 cubic inches; the whole of which space was occupied by the substances by which the bulb of the thermometer was surrounded in the experiments in question.

But though these substances occupied this space, they were far from filling it; by much the greater part of it being filled by the air which occupied the interstices of the substances in question. In the experiment N° 4, this space was occupied

by 16 grains of raw silk; and as the specific gravity of raw silk is to that of water as 1734 to 1000, the volume of this silk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,185}$=0,037294 of a cubic inch; and, as the space it occupied amounted to 2,05755 cubic inches, it appears that the silk filled no more than about $\frac{1}{55}$ part of the space in which it was confined, the rest of that space being filled with air.

In the experiment N° 1, when the space between the bulb of the thermometer and the glass globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was 576 seconds; but in the experiment N° 4, when this same space was filled with 54 parts air, and 1 part raw silk, the time of cooling was 1284 seconds.

Now, supposing that the silk had been totally incapable of conducting any heat at all, if we suppose, at the same time, that it had no power to prevent the air remaining in the globe from conducting it, in that case its presence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had displaced to the quantity remaining, that is to say, as 1 is to 54, or a little more than 10 seconds. But the time of cooling was actually prolonged 708 seconds, (for in the experiment N° 1, it was 576 seconds, and in the experiment N° 4, it was 1284 seconds, as has just been observed); and this shows, that the silk not only did not conduct the heat itself, but that it prevented the air by which its interstices were filled from conducting it; or, at least, it greatly weakened its power of conducting it.

The next question which arises is, how air can be prevented from conducting heat? and this necessarily involves another, which is, how does air conduct heat?

If air conducted heat, as it is probable that the metals and water, and all other solid bodies and unelastic fluids conduct it, that is to say, if its particles remaining in their places, the heat passed from one particle to another, through the whole mass, as there is no reason to suppose that the propagation of heat is necessarily in right lines, I cannot conceive how the interposition of so small a quantity of any solid body as $\frac{1}{55}$ part of the volume of the air, could have effected so remarkable a diminution of the conducting power of the air, as appeared in the experiment (N° 4) with raw silk, above mentioned.

If air and water conducted heat in the same *manner*, it is more than probable that their conducting powers might be impaired by the same means; but when I made the experiment with water, by filling the glass globe, in the centre of which the bulb of the thermometer was suspended, with that fluid, and afterwards varied the experiment, by adding 16 grains of raw silk to the water, I did not find that the conducting power of the water was sensibly impaired by the presence of the silk.

But we have just seen that the same silk, mixed with an equal volume of air, diminished its conducting power in a very remarkable degree; consequently, there is great reason to conclude that water and air conduct heat in a *different manner*.

But the following experiment, I think, puts the matter beyond all doubt.

It is well known, that the power which air possesses of

holding water in solution is augmented by heat, and diminished by cold, and that, if hot air is saturated with water, and if this air is afterwards cooled, a part of its water is necessarily deposed. I took a cylindrical bottle of very clear transparent glass, about 8 inches in diameter, and 12 inches high, with a short and narrow neck, and suspending a small piece of linen rag, moderately wet, in the middle of it, I plunged it into a large vessel of water, warmed to about 100° of Fahrenheit's thermometer, where I suffered it to remain till the contained air was not only warm, but thoroughly saturated with the moisture which it attracted from the linen rag, the mouth of the bulb being well stopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and stopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing just so much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine transparent glass, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could see what passed in the bottle most distinctly; and having taken care to place the jar upon a table near the window, in a very favourable light, I set myself to observe the appearances which should take place, with all that anxious expectation which a conviction that the result of the experiment must be decisive, naturally inspired.

I was certain, that the air contained in the bottle could not part with its heat, without at the same time, that is to say, *at the same moment*, and *in the same place*, parting with a proportion

of its water; if, therefore, the heat penetrated the mass of air from the centre to the surface, or *passed through it* from particle to particle, in the same manner as it is probable that it passes through water, and all other unelastic fluids, by far the greatest part of the air contained in the bottle would part with its heat, when *not actually in contact with the glass*, and a proportional part of its water being let fall at the same time, and in the *same place*, would necessarily descend in the form of rain; and, though this rain might be too fine to be visible in its descent, yet I was sure I should find it at the bottom of the bottle, if not in visible drops of water, yet in that kind of cloudy covering which cold glass acquires from a contact with hot steam or watery vapour.

But if the particles of air, instead of communicating their heat from one to another, from the centre to the surface of the bottle, each in its turn, and for itself, came to the surface of the bottle, and there deposited its heat, and its water, I concluded that the cloudiness occasioned by this deposit of water would appear all over the bottle, or, at least, not more of it at the bottom than at the sides, but rather less; and this I found to be the case in fact.

The cloudiness first made its appearance upon the sides of the bottle, near the top of it; and from thence it gradually spread itself downwards, till, growing fainter as it descended lower, it was hardly visible at the distance of half an inch from the bottom of the bottle; and upon the bottom itself, which was nearly flat, there was scarcely the smallest appearance of cloudiness.

These appearances, I think, are easy to be accounted for. The air immediately in contact with the glass being cooled, and

having deposited a part of its water upon the surface of the glass, at the same time that it communicates to it its heat, slides downwards by the sides of the bottle in consequence of its increased specific gravity, and, taking its place at the bottom of the bottle, forces the whole mass of hot air upwards; which, in its turn coming to the sides of the bottle, *there* deposites its heat and its water, and afterwards bending its course downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the first appearance of condensed vapour is near the top of the bottle, as also why the greatest collection of vapour is in that part, and that so very small a quantity of it is found nearer the bottom of the bottle.

This experiment confirmed me in an opinion which I had for some time entertained, that, though the particles of air individually, or each for itself, are capable of receiving and *transporting* heat, yet air in a quiescent state, or as a fluid whose parts are at rest with respect to each other, is not capable of conducting it, or giving it a passage; in short, that heat is incapable of *passing through a mass of air*, penetrating from one particle of it to another, and that it is to this circumstance that its non-conducting power is principally owing.

It is also to this circumstance, in a great measure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining heat, is so remarkably increased upon being mixed with a small quantity of any very fine, light, solid substance, such as the raw silk, fur, Eider down, &c. in the foregoing experiments: for as I have already observed, though these substances, in the very small quantities in which they were made use of, could hardly have prevented,

in any considerable degree, the air from conducting, or giving a *passage* to the heat, had it been capable of passing through it, yet they might very much impede it in the operation of transporting it.

But there is another circumstance which it is necessary to take into the account, and that is the attraction which subsists between air and the bodies above mentioned, and other like substances, constituting natural and artificial clothing. For, though the incapacity of air to give a passage to heat in the manner solid bodies and non-elastic fluids permit it to pass through them, may enable us to account for its warmth under certain circumstances, yet the bare admission of this principle does not seem to be sufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing; nor even that which we find in snow; for if we suppose the particles of air to be at liberty to *carry off* the heat which these bodies are meant to confine, without any other obstruction or hinderance than that arising from their *vis inertiæ*, or the force necessary to put them in motion, it seems probable that the succession of fresh particles of cold air, and the consequent loss of heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very strong one, actually subsists between the particles of air, and the fine hair or furs of beasts, the feathers of birds, wool, &c. appears by the obstinacy with which these substances retain the air which adheres to them, even when immersed in water, and put under the receiver of an air-pump; and that this attraction is essential to the warmth of these bodies, I think is very easy to be demonstrated.

In furs, for instance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increased elasticity, or repulsion of those particles with regard to each other, arising from the heat communicated to them by the animal body, the air in the fur, though heated, is not easily displaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the heat of the animal, because it is itself confined, by its attraction to the hair or fur; and it transmits it with great difficulty, if it transmits it all, as has been abundantly shown by the foregoing experiments.

Hence it appears why those furs which are the finest, longest, and thickest, are likewise the warmest; and how the furs of the beaver, of the otter, and of other like quadrupeds which live much in water, and the feathers of water-fowls, are able to confine the heat of those animals in winter, notwithstanding the extreme coldness and great conducting power of the water in which they swim. The attraction between these substances, and the air which occupies their interstices, is so great, that this air is not dislodged even by the contact of water, but remaining in its place, it defends the body of the animal at the same time from being wet, and from being robbed of its heat by the surrounding cold fluid; and it is possible that the pressure of this fluid upon the covering of air confined in the interstices of the fur, or feathers, may at the same time increase its warmth, or non-conducting power, in such a manner that the animal may not, in fact, lose more heat when in water, than when in air: for we have seen by the foregoing experiments, that, under certain circumstances, the warmth of a covering is increased, by bringing its component parts nearer together, or by

increasing its density even at the expence of its thickness. But this point will be further investigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interstices of the hairs of the animal tending naturally to rise upwards, in consequence of its increased elasticity, would escape with much greater ease from the backs of quadrupeds than from their bellies, had not Providence wisely guarded against this evil by increasing the obstructions in those parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almost to a proof of the principles assumed relative to the manner in which heat is carried off by air, and the causes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining heat.

The snows which cover the surface of the earth in winter, in high latitudes, are doubtless designed by an all-provident Creator as a garment to defend it against the piercing winds from the polar regions, which prevail during the cold season.

These winds, notwithstanding the vast tracts of continent over which they blow, retain their sharpness as long as the ground they pass over is covered with snow; and it is not till, meeting with the ocean, they acquire, from a contact with its waters, the heat which the snows prevent their acquiring from the earth, that the edge of their coldness is taken off, and they gradually die away and are lost.

The winds are always found to be much colder when the ground is covered with snow than when it is bare, and this

extraordinary coldness is vulgarly supposed to be communicated to the air by the snow; but this is an erroneous opinion; for these winds are in general much colder than the snow itself.

They retain their coldness, because the snow prevents them from being warmed at the expence of the earth; and this is a striking proof of the use of the snows in preserving the heat of the earth during the winter, in cold latitudes.

It is remarkable that these winds seldom blow from the poles directly towards the equator, but from the land towards the sea. Upon the eastern coast of North America the cold winds come from the north-west; but upon the western coast of Europe, they blow from the north-east.

That they should blow towards those parts where they can most easily acquire the heat they are in search of, is not extraordinary; and that they should gradually cease and die away, upon being warmed by a contact with the waters of the ocean, is likewise agreeable to the nature and causes of their motion; and if I might be allowed a conjecture respecting the principal use of the seas, or the reason why the proportion of water upon the surface of our globe is so great, compared to that of the land, it is to maintain a more equal temperature in the different climates, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon passing over the sea, and that hot winds are refreshed by a contact with its waters, is very certain; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than those which blow from the land.

In the islands of Great Britain and Ireland, there is not the least doubt but the great mildness of the climate is entirely

owing to their separation from the neighbouring continent by so large a tract of sea; and in all similar situations, in every part of the globe, similar causes are found to produce similar effects.

The cold north-west winds, which prevail upon the coast of North America during the winter, seldom extend above 100 leagues from the shore, and they are always found to be less violent, and less piercing, as they are further from the land.

These periodical winds from the continents of Europe and North America prevail most towards the end of the month of February, and in the month of March; and I conceive that they contribute very essentially towards bringing on an early spring, and a fruitful summer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with snow. The whole atmosphere of the polar regions being, as it were, transported into the ocean by these winds, is there warmed and saturated with water: and, a great accumulation of air upon the sea being the necessary consequence of the long continuance of these cold winds from the shore, upon their ceasing the warm breezes from the sea necessarily commence, and, spreading themselves upon the land far and wide, assist the returning sun in dismantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the sea, having acquired its heat from a contact with the ocean, is, of course, saturated with water; and hence the warm showers of April and May, so necessary to a fruitful season.

The ocean may be considered as the great reservoir and equalizer of heat; and its benign influences in preserving a proper temperature in the atmosphere operate in all seasons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the sea, which, at certain hours of the day, come in to the shores in almost all hot countries, bring with them refreshment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the excessive heats of a burning sun. What a vast tract of country, now the most fertile upon the face of the globe, would be absolutely barren and uninhabitable on account of the excessive heat, were it not for these refreshing sea-breezes? And is it not more than probable, that the extremes of heat and of cold in the different seasons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preserving an equability of temperature?

And to these purposes the ocean is wonderfully well adapted, not only on account of the great power of water to absorb heat, and the vast depth and extent of the different seas (which are such that one summer or one winter could hardly be supposed to have any sensible effect in heating or cooling this enormous mass); but also on account of the continual circulation which is carried on in the ocean itself, by means of the currents which prevail in it. The waters under the torrid zone being carried by these currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their heat to these inhospitable regions,

return towards the equator, carrying with them refreshment for those parching climates.

The wisdom and goodness of Providence have often been called in question with regard to the distribution of land and water upon the surface of our globe, the vast extent of the ocean having been considered as a proof of the little regard that has been paid to man in this distribution. But, the more light we acquire relative to the real constitution of things, and the various uses of the different parts of the visible creation, the less we shall be disposed to indulge ourselves in such frivolous criticisms.

V. *A new Suspension of the Magnetic Needle, intended for the Discovery of minute Quantities of Magnetic Attraction: also an Air Vane of great Sensibility; with new Experiments on the Magnetism of Iron Filings and Brass.* By the Rev. A. Bennet, *F. R. S. Communicated by the Rev. Sir* Richard Kaye, *Bart. F. R. S.*

Read January 26, 1792.

To manifest the various degrees of attraction between magnets and ferruginous bodies, different methods have been used. The substance to be tried has either been simply brought into contact with the magnet, or has been made to float on water or mercury. Needles are commonly made to rest horizontally on sharp-pointed wires, and as an improvement on these methods, Mr. Cavallo has suspended a needle by a chain of horse-hair, consisting of five or six links, which move very freely in each other, and allow the needle to turn more than a whole revolution round its centre. By comparing this needle with others of the best sort in use, he found it much more sensible. Others have suspended the needle by fine threads, or silk: but as these, on turning round a few times, will cause the needle to deviate from its meridian by twisting, they are certainly objectionable.

After considering each of the above methods, and trying some of them in November, 1789, I suspended a small sewing

needle, by means of a spider's thread, in the cylindrical glass of my gold-leaf electrometer; and having satisfactorily proved its magnetic sensibility, I now venture to propose this kind of suspension, as being well adapted to experiments requiring the needle to move with the least resistance.

EXPERIMENT I.

From the astonishing tenuity of a spider's thread (see BAKER's Microscope made easy, Part II. Ch. XXV.), it might be expected that it would bear very much twisting, without causing the needle to be sensibly drawn from its magnetic meridian: but to prove it more fully by direct experiments, I first fastened a small hair to the side of the glass in which the needle was suspended, and placed it so that the point of the needle stood exactly opposite to the point of the hair. Then I turned the needle round, by means of a magnet, about 800 times, and on removing the magnet, I found that the needle rested exactly opposite to the hair: thus a spider's thread only two inches long, by twisting 800 times, did not cause any sensible deviation.

EXPERIMENT II.

A fine harpsichord wire, three inches long, was suspended in a larger glass. This wire was previously rendered magnetic by making it red hot in the flame of a candle, and suffering it to cool in the direction of the magnetic meridian: whereby it acquired polarity by the influence of the earth's magnetic atmosphere alone, and being soft, it possessed (as might be expected) but a weak directive power. The spider's thread was three inches long, and a small hair was fastened

by varnish to the north pole of this wire, which served more accurately to distinguish its position opposite to a bit of ivory marked with degrees. This wire was turned round as before, more than a thousand times; yet when suffered to rest, it stood exactly at the same degree, the twist of the spider's thread having produced no sensible deviation.

EXPERIMENT III.

A fine spider's thread was fastened to the spindle of a wheel used for spinning flax; the wheel was placed so that the spindle and thread might hang perpendicularly. To the end of the thread, which was about two inches and an half long, was fastened, by its smaller end, one fibre of the feather of a goose-quill: the lower end of the fibre rested upon a book. The wheel was turned round till the spindle had made above 18,000 revolutions. During this time the spider's thread gradually became about one inch shorter, yet all this twisting did not cause the fibre to turn round when raised from the book. On turning the spindle about 500 times more, the thread broke, apparently by twisting.

Several twisted spider's threads were viewed by means of a very good lucernal microscope, made by Mr. ADAMS, without being able to perceive any signs of twist, except where it was doubled, and which appeared like hairs growing out of the thread: yet the twist may be distinctly seen by the naked eye, in the sunshine, in some threads to be found amongst trees, where the spider has probably been turned round by the wind during its descent from one branch to another.

EXPERIMENT IV.

A bristle was suspended horizontally by a spider's thread somewhat stronger than the last, and after turning the wheel till it produced 4800 revolutions, it shortened the thread from three inches to one inch; yet either end of the bristle would move towards any warm substance which was presented to it, either with or against the direction of the twist.

EXPERIMENT V.

Several other light substances were suspended by fine spider's threads, and placed in a cylindrical glass about two inches in diameter, as the thinnest part of the wing of a dragon-fly, thistle down, and the down of dandelion; of these, the last appeared most sensible to the influence of heat, for when this down was fastened to one end of a fine gold wire suspended horizontally, or to one end of two bits of straw joined together in the form of the letter T inverted, it would turn towards any person who approached it at the distance of three feet, and would move so rapidly towards wires only heated by my hand, as very much to resemble magnetic attraction. See Tab. II. fig. 1.

EXPERIMENT VI.

A bottle filled with cold water was brought near the glass cylinder, standing in a warm room, and soon after the down of dandelion appeared to be repelled by the bottle, by turning away from it. The bottle was removed to the other side, and the dandelion again moved towards the opposite side.

These attractions and repulsions, since they acted through

glass, and at so great a distance, appeared to me at first to be the effects of atmospheres of heat, acting in a similar manner to those of electricity; and on shewing them to a believer in animal magnetism, he was confident that the light substances were moved by the magnetic atmosphere of the body, and that there was a difference betwixt the attraction of the right hand and the left: but the doubts of my philosophical friends induced me to try the following experiment, which fully explains the matter.

EXPERIMENT VII.

A piece of paper was tied over the mouth of a glass jar about four inches in diameter. Two holes were made in the paper opposite to each other, and near the edge of the glass. The jar was placed upon a table, and suffered to stand a considerable time to cool in a room without fire; I then sat near it, on the side where one of the holes in the paper was in the nearer, and the other in the farther end of the diameter. I next filled another glass with smoke, and placed it with its mouth over the two holes in the paper. The smoke was now seen to descend through the farthest hole, and mixing with the air in the lower jar, plainly shewed, that the air moved slowly towards the side of the glass warmed by the heat of my body.

EXPERIMENT VIII.

The last experiment proved, that the current of air moving towards, and then ascending up the heated side of the glass, might turn the broader end of the light suspended body, like the vane of a weathercock: but that it might still more decisively appear to be occasioned by a current of heated air, I

suspended three inches of gold wire $\frac{1}{280}$ of an inch thick, by a spider's thread; about an inch of the same wire was fastened to the middle, and hanging perpendicularly from the place to which the thread was attached, kept it in an horizontal position whilst undisturbed, and yet suffered it to move up and down like a scale beam, by a small degree of motion in the air. Under one end of this wire heated substances were introduced, which caused it to move upwards, seeming to be repelled by them with as much force, as it was attracted when they were applied horizontally.

Having found that a spider's thread, only two inches and an half long, when twisted by above 18,000 revolutions, would not cause a sensible deviation of the magnetic needle, owing to its very great tenuity, or to its glutinous quality preventing its having any tendency to untwist; and that light substances suspended by it, and inclosed in a glass, were capable of being turned about by so small a degree of heat as that occasioned by a person sitting at the distance of three feet from the instrument; or by wires, or other substances, only warmed by holding in my hand; and that when the instrument was placed in a cool room, a slight touch with the end of my finger would cause the wing of the dragon-fly, or even a bit of straw, to point exactly at the side of the glass which had been touched; there could remain no doubt of the freedom with which a magnetic needle would move when thus suspended: yet another experiment more directly proves its freedom of motion to be greater than that of former methods.

EXPERIMENT IX.

Six rings of horse-hair, made exactly according to Mr.

CAVALLO's direction, were suspended in a cylindrical glass jar; to the lowest of these rings a spider's thread, three inches long, was attached. This thread was fastened to a gold wire twisted round the middle of a small sewing needle. The jar was placed with its mouth downwards, and over the edge of a table, the needle hanging a little lower. After the needle and rings of horse-hair were perfectly at rest, the point of the needle was struck with the end of my finger, which caused it to turn round very swiftly, yet this twisting did not move the rings of horse-hair. An harpsichord wire, twenty-one inches long, was suspended by ten spider's threads, to the lowest ring of the horse-hair chain; this was also frequently turned round without moving the rings. A wire of this length was afterwards suspended by spider's threads in a proper frame, and with an ivory scale of degrees, with an intention to observe the daily variation; but it was too much influenced by heat, which I have not yet been able to obviate.

EXPERIMENT X.

To the end of a fine gold wire, three inches long, and suspended by a spider's thread in a cylindrical glass, was fastened a small circular bit of writing paper; light was admitted through a small hole, and also the focus of a large lens was thrown upon the paper, with the intention of observing whether it would be moved by the impulse of light: but though these experiments were often repeated, and once with the paper suspended in an exhausted receiver, yet I could not perceive any motion distinguishable from the effects of heat. Perhaps sensible heat and light may not be caused by the influx or rectilineal projections of fine particles: but by the

vibrations made in the universally diffused *caloric* or matter of heat, or fluid of light. I think modern discoveries, especially those of electricity, favour the latter hypothesis.

EXPERIMENT XI.

When cold wires are introduced within the glass jar, and near the light suspended substances, as bits of straw, or very fine wires, a very slight motion of the introduced substances pushes the suspended wires at the distance of an inch, appearing like repulsion, but a contrary motion draws them nearer; therefore it may be attributed to the motion of the air. Also, when a spider's thread, without any thing attached to it, is suspended in a dry glass jar, it recedes from the electric atmosphere of an excited glass tube brought suddenly near the outside of the glass jar, whether the tube be positively or negatively electrified; and when the tube is suddenly removed, the thread is drawn after it. This may be accounted for by the difficulty with which the spider's thread changes its state of electricity, being a very imperfect conductor; whence also, notwithstanding its extreme fineness, it is unfit to be used as an electrometer.

EXPERIMENT XII.

It is indispensibly necessary that a magnetic needle, intended for the discovery of minute quantities of magnetic attraction, should be inclosed in a proper apparatus, that the motion of the air may not disturb it, and that the substances to be tried may be brought sufficiently near, and at right angles with the point of the needle. The following instrument was constructed for this purpose, and is submitted to the approbation

of those who are desirous of making further improvements in the science of magnetism. In fig. 2. A is the bottom of the instrument, $5\frac{1}{2}$ inches square, and one inch thick, to lie horizontally on the table, and made of mahogany. B B is a frame of the same wood, standing vertically on the middle of the square bottom, $6\frac{1}{2}$ inches high, and $5\frac{1}{2}$ broad. On one side of the frame a piece of glass is fixed, and a piece of writing paper, gold beater's skin, or other thin substance, is pasted to the edge of the frame on the other side, so that the glass and the paper stand vertically and parallel to each other, at the distance of about half an inch, which is sufficient room for the needle, hanging by a spider's thread from the screw at C, which screw passes through the top piece D, which drops into an opening cut through the frame, and may be taken out by means of the knob of the screw at E. About 10 degrees of a circle are marked upon a bit of ivory fastened to the edge of the frame at F, and the needle, three inches long, is made of the smallest steel harpsichord wire, and is suspended by a spider's thread, also three inches long. A small tapering hair, fastened to the north pole with varnish, and extending about $\frac{1}{8}$ of an inch farther than the end of the wire, points at the degrees on the ivory, so that its motion may be very accurately distinguished, and especially by the use of a lens. Round the middle of the needle a small gold wire is twisted, and its end standing perpendicularly is fastened to the spider's thread, to keep the needle in a horizontal position.

The method of obtaining and fastening the spider's thread, which I have used, is this:—I take a forked stick, consisting of a stem and two or three branches, standing at about six inches asunder at their ends, and after finding a proper thread, in the

corner of a building, or amongst trees, the ends of the stick are smeared with varnish, and each end applied to the thread, which being turned round the ends of the stick, may be brought away: or several forked sticks may be placed in a room, where spiders are not disturbed, and they will soon attach their threads to them.

When the thread is in this manner extended between the branches, the stick is placed upon the edge of a table, and in the sunshine, if possible, that it may be in a convenient position for attaching it to the needle and screw, by which it is to hang. The gold wire above mentioned is then dipped in varnish, and applied to one end of the spider's thread, so as to entangle it in the varnish, which soon drying, the thread becomes fast, and may be suffered to hang down, the upper end of the thread being yet fast to the forked stick. The point of the screw in the top piece is then dipped in varnish, and the upper end of the thread attached to it in the same manner. Lastly, the needle is gradually lowered through the opening in the top of the instrument, till it hangs opposite to the marked ivory: but if the thread be too long, it is easily shortened, by lifting up the top piece, touching the end of the screw with varnish, and turning it round, so as to wind the thread about it, till it will hang near the edge of the ivory: but to set the point of the hair at the end of the needle at a more exact distance, after the top piece is replaced, the screw is turned by its knob E, which raises or lowers the needle till it hangs right. A second method of raising or lowering the needle, with less trouble in the construction, is to use a small brass wire instead of the screw, which may pass through a cork; or the ivory index may be made to rise and fall by means of a

screw. If both sides of the instrument be made of glass, it may be filled with some transparent fluid, which will not cause the metal to rust, as spirit of wine, or oil of turpentine, that the needle may move more steadily, and not be sensibly influenced by heat.

Expedients for rendering the instrument more portable might be easily contrived: but I hope no unprejudiced philosopher will pronounce it useless, because the spider's thread, which is so easily replaced, may break; for sensibility is the only property of essential use in discoveries wherein a small degree of magnetic attraction is to be ascertained.

EXPERIMENT XIII.

The first use I made of my needle, suspended as above, was to try the polarity of several iron utensils; and, as might be expected, they attracted or repelled the north end of the needle, according to their position with respect to the magnetic atmosphere of the earth. A bar of soft iron, half an inch square, and nine inches long, moved the needle very sensibly at the distance of about three feet; longer bars moved it at a much greater distance; and if a bar was held horizontally, near the end of the needle, and at right angles, it might be made either to attract or repel, by moving it up or down only half an inch, so as to appear to change its attraction to repulsion at command; which has surprised persons unacquainted with the importance of position with respect to the earth's magnetic atmosphere. This polarity of position may be very sensibly perceived, by presenting small nails, or smaller bits of wire, above or below the needle, or with the remote end inclining towards the north or south; which plainly demonstrates the

existence of a magnetic atmosphere over the earth, where the magnetic fluid being rarefied at one pole, and condensed at the other, occasions the polar direction of the needle, of so much use in navigation.

EXPERIMENT XIV.

At the request of Dr. DARWIN, I repeated an experiment of Mr. CAVALLO, on the increased attraction of iron filings by effervescence with diluted vitriolic acid, inserted in the Philosophical Transactions, Vol. LXXVII. and according to his direction, I placed two ounces of iron filings in an earthen vessel, near the south end of the needle, and first pouring in five parts of water, and then one of good vitriolic acid, a brisk effervescence ensued; but the needle could not be perceived to move. This I repeated six times, sometimes supposing it moved a little, but was not certain, and despaired of success till I recollected of how much importance it was to place the filings in a proper direction as to the earth's magnetic atmosphere; for hitherto, although the filings were placed at right angles with the point of the needle, yet the bottom of the vessel was as much below as the upper surface of the filings was above it. I then took a quantity of filings tied up in a paper, and presenting them to the needle, I found that the bottom always repelled, and the top attracted the north pole; it was therefore necessary to place the bottom of the vessel even with the needle, or rather above it, which circumstance Mr. CAVALLO did not mention. I then placed the vessel at such a distance, that the needle was drawn one degree from its former position; then on adding the water and vitriolic acid as before, the needle came about one degree still nearer; yet it appeared

improbable that this increased attraction should be owing to any chemical change in the filings; and as it was so necessary to place them right, it appeared to me likely that whilst each particle of iron was emitting bubbles of inflammable air, and thereby increasing the perpendicular height of the filings, this circumstance would prove the only cause of such increased attraction. To verify this, I put a quantity of filings into an earthen vessel, which covered the bottom an inch deep; then placing them so as to draw the needle two degrees from its meridian, I marked exactly where the vessel stood, and taking it away, I mixed with the filings a quantity of sand, which raised the whole depth of the mixture to two inches; then replacing the vessel, I found the needle drawn above four degrees from its meridian. Still more to confirm this, I placed the filings below instead of above the south pole, and repeated both the effervescence and mixture of sand, and thereby caused as great an increase of repulsion, as of attraction by placing them above. Each of these experiments was repeated, by placing the vessel above and below the north pole also, and the results were accordingly varied: but when small nails were used instead of the filings, no increased attraction was produced, because the nails were too heavy to be raised higher in the vessel, although the effervescence was very violent; whence I was fully convinced that this experiment depended only on raising the perpendicular height of the column of iron filings.

EXPERIMENT XV.

To obviate the suspicion that effervescence might agitate the iron filings, and bring a greater number of them to that side of the vessel which stands contiguous to the magnetic

needle, Mr. CAVALLO repeated the experiment with six yards of steel wire twisted in various directions, so as to be admitted into the pot; in which case the metal presented a large surface to the acid, and was not subject to be moved by the effervescence. On adding the diluted vitriolic acid, the needle came somewhat more than half a degree nearer to the pot. After considering the influence of heat in the above experiments, and that the motion of the needle was only half a degree, it may reasonably be suspected to be occasioned by the heat of the vessel: yet I think it may also be accounted for by the change of magnetism in the twisted wire; for, after trying several times to produce this attraction, by pouring the diluted vitriolic acid on a number of bits of wire tied to the end of a glass tube without twisting, I could not produce any increased attraction; at last, by twisting the wire, I succeeded to produce a degree of attraction, which I thought was owing to the expansion or untwisting of the wire during the effervescence. This opinion was strengthened by the changes of polarity I could so easily produce in softened steel wire; first by passing it through a candle, by which it would acquire a very sensible polarity, according to the position in which it was cooled; secondly, by slightly drawing it through my fingers, held in different directions in respect to the earth's polarity; thirdly, when a wire, three inches long, was fastened to the end of a glass tube, and deprived of sensible polarity, by passing through a candle at right angles to the magnetic meridian, it would become possessed of sensible polarity by a small blow with my finger on the glass tube, whilst held in a proper position; fourthly, the smallest degree of pressure between my fingers, so as to bend the wire, would give it polarity. Now,

a quantity of harpsichord wire bent in a great number of directions, will possess in some parts a polarity attractive, and in others repulsive, of the south pole of the needle; and when placed in an earthen vessel, with its bottom somewhat above the needle, it attracts the south pole, first by those parts which have acquired a north polarity by bending; and this attraction is strengthened by position; and also position gives north polarity to other parts, in other respects indifferent: then, during the effervescence, the heat of the mixture dilates the wire, and in some places thereby bends it one way or other, which occasions it to acquire the magnetism of position, whence there must sometimes result an increased attraction. From these and other experiments it appears, that whenever the particles of steel (hard enough to be capable of fixed polarity) are by any means displaced, they admit their natural magnetism to become rarefied and condensed by the influence of the earth's magnetic atmosphere; and thus the effect of electrical shocks on steel wires may be explained: but it does not appear probable that the action of acids increases the magnetic attraction of iron, unless accompanied by other circumstances, to which an increased attraction may be more reasonably attributed.

EXPERIMENT XVI.

On reading Mr. CAVALLO's experiments on the increased attraction of iron by effervescence, Dr. DARWIN was led to inquire, whether inflammable air be magnetic. I therefore, at his request, caused inflammable air to issue through a paper tube held near the north and south pole of the needle alternately; the air was also received in a bladder, and

applied; but without producing any sensible effect on the needle.

In the LXXVIth Volume of the Philosophical Transactions, Mr. CAVALLO has endeavoured to prove, that brass "does not owe its magnetism to iron, but to some particular "configuration of its component particles, occasioned by the "usual method of hardening it, which is by hammering." Some brass, he observes, will not acquire "any sensible mag-"netism by hammering." And in other pieces, which have often passed from the workshop to the furnace, and from the latter to the former, there is contained iron, which renders them magnetic.

Now, since some brass is evidently magnetic because it contains iron, it appears to me likely that brass, whose magnetism is made sensible by hammering, contains a smaller quantity of iron, and that hammering renders it sensible, by giving it some degree of polarity. Therefore no brass can acquire this property which contains no iron. This will appear more evident by the following experiments.

EXPERIMENT XVII.

I placed an iron nail, about two inches long, in the fire, where it became red hot, and cooled, as the fire went out, in a position east and west with respect to the magnetic meridian; by which it became very soft, and when presented towards the needle, it attracted or repelled according to its position, having no fixed polarity. The nail was then placed upon an anvil, with the point directed towards the south of the magnetic meridian; and after hammering in this position till it was considerably hardened, its point possessed a fixed

south polarity; the other end, being thicker, did not seem to be altered.

Another nail was hammered with its point towards the north, which gave it a fixed north polarity. The polarity of these hammered nails might be instantly changed, by bending the point, whilst held in a contrary position to that in which they were hammered. Several oblong pieces of magnetic brass were hammered in the same manner, and thereby made to possess a north or south polarity, according to their position whilst hammered: hence it appears, that the general effect of hammering is to harden the metal, whereby it becomes in some degree a non-conductor of magnetism, and retains that rarefied and condensed, and therefore more sensible, state of the fluid, which is produced by the influence of the earth's magnetic atmosphere.

EXPERIMENT XVIII.

In a small crucible I placed six thin plates of copper, and betwixt each of them a plate of zinc; these being melted, and cast in a proper mould, produced an oblong piece of brass, which was not sensibly magnetic, nor could I produce any magnetism in it by hammering. The same quantity of copper and zinc were melted, with the addition of some small bits of iron. This brass was very sensibly magnetic, and, when hammered, acquired polarity, by which it more sensibly attracted or repelled the needle. Lastly, a piece of copper was melted, with the addition of some iron, which was also sensibly magnetic. From these experiments I conclude, that brass owes its magnetism to iron; but that it may sometimes contain so small a quantity as not to be sensible till it be hammered.

The greater sensibility, and easy construction, of a magnetic needle, suspended by a spider's thread, as above described, I hope, will contribute to extend magnetic observations, which may in time produce a true theory of this important branch of science; while the very sensible air vane, from the same mode of suspension, may prevent the apparent attractions and repulsions, occasioned by currents of air, from being confounded with those of magnetism.

Fig. 1.

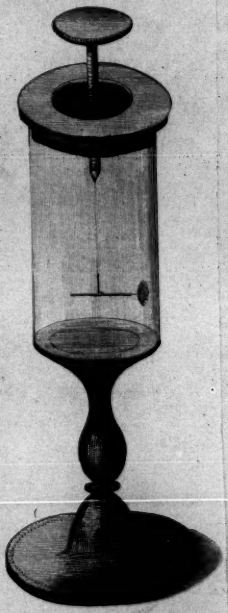
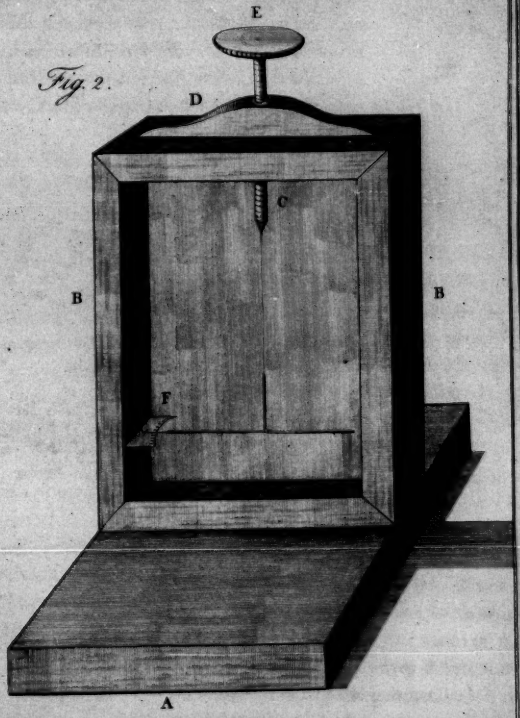


Fig. 2.



Basire Sc.

VI. *Part of a Letter from Mr.* Michael Topping, *to Mr.* Tiberius Cavallo, *F.R.S.*

Read February 16, 1792.

DEAR SIR, *Madras, February 4, 1789.*

I INCLOSE you an account of a base line I have measured for a series of triangles I am carrying down the coast of Coromandel. I have already extended them to about 300 miles from Madras, and am upon returning back to prosecute the work quite down to Cape Comorin. The angles are all taken with my Hadley's sextant, made by STANCLIFFE, by means of three tall signals I have constructed of bamboos, 80 feet high, 60 of which I mount upon steps, so as to see (over all trees, &c.) very distinctly my two other signals, at the distance of from 8 to 13 miles. It is, I believe, the first time the Hadley was ever made use of for a purpose of such magnitude; but it is fully equal to it—nay, it does more—the sun's bearing, or oblique distance, from my signals is also taken by it; by which, and his azimuth (computed) I obtain the angles made by them with the meridian; and by combining the whole, the difference of latitude, and meridional distance of every one of them in English fathoms. This is found so nicely, that a mean of my astronomical observations for the latitudes, never differs more than a few seconds from those given by the geometrical mensuration. In all the operations I have had no one to assist me,

except a party of black fellows to carry my flags. I need not tell you how many thousand miles I have travelled to take the angles; nor what the labour and fatigue of such a work must be in this burning climate, where I have frequently had the thermometer at 106° in my tent.

(Signed) M. TOPPING.

An Account of the Measurement of a Base Line *upon the Sea Beach, near* Porto Novo, *on the* Coast of Coromandel, *in May*, 1788; *by Mr.* Michael Topping.

As a necessary foundation for the chain of triangles now carrying on, I have, for some time past, had my thoughts bent towards measuring, with all possible accuracy, a *base line*. This base line, could I have chosen its situation, should have been determined as near the middle of the line of coast I am surveying as possible; but circumstances have not permitted me to make unrestrained choice of its place. On my arrival at Cuddalore, I was told that, as I proceeded southward, I should meet with frequent rivers, and other water courses, that would certainly obstruct me in the design I had formed of measuring it on the sea beach, farther south; and, soon after my removal from that place, I found, with much satisfaction, that the coast, between Cuddalore river and Porto Novo, would serve my purpose extremely well. The beach hereabouts is flat, broad, and remarkably smooth; the only specious objection that can be made to it, is its not being straight, but forming a curved line, concave towards the sea. This, however, I knew to be, in reality, of no bad consequence, since several right

lines of sufficient length might, I perceived, be measured upon it; the angles they might interchangeably make, be taken; and the whole afterwards be reduced to one direct line by calculation.

These considerations, and an accident that, about the same time, befel one of my signals, and delayed my trigonometrical progress, finally determined me to measure my base at this opportunity; and I accordingly began that work by placing two of my large signals, at the distance (as nearly as I could judge) of seven miles asunder, for the terms or extremities of it.

In traversing the coast between the signals, I found that, in order to continue my operations throughout upon the most advantageous part of the beach, I must divide the whole distance into six distinct portions, each portion forming a large angle with the next portion to it: and this I immediately did, by placing five lesser flags, at intervals convenient for that purpose.

I now began my measurement, in prosecuting which I spared neither pains nor care. The two rods, of 25 feet each, which I had provided for this use, had been strictly examined while I was at Cuddalore, and their lengths ascertained, as exactly as the conveniences this country affords would admit of. They had been left *purposely* a little too long, as I found it easier to determine, and allow for such excess, than to reduce them to sufficient exactness. In settling their respective lengths, a capital two-feet brass sector, by Adams, was made use of, as a standard: this instrument was not only the best authority I could procure; but, being my own property, would, I considered, be ready for an examination, should an opportu-

nity arise of comparing it, in future, with any measure of more acknowledged credit. The thermometer stood at 87°, when the rods were adjusted by it.

The stands, which I had prepared for levelling the rods, were also brought out; and it was with much regret that I found I could not profit by them, as I had hoped to do, assisted as I was by none but black people, in whom, I perceived, it would have been impossible, without incurring great loss of time, to have impressed a necessary idea of their nature and management. I therefore resolved to dispense with the stands, and to lay the rods, end to end, upon the ground. It was in a similar way that the base line for a series of triangles, continued throughout France, was measured. The French rods, which were nearly of the same length and construction with mine, were disposed, in the very same manner, upon the rugged pavement of a highway near Paris; so that I have every reason to believe the opportunity here afforded me, of a peculiarly level and sandy beach, to be the best of the two.

The mode of conducting the measurement was this:—Staves were first set up, in a direct line, between the flags; from every two of these staves a rope was occasionally stretched, as tight as possible, on the ground, and the rods were laid by the side of the rope. The first rod being properly placed, the second was laid *near its end*, and then very carefully adjusted, so as to touch the ferrule of the other, by a man, who had no other employment to engage his attention; and in the performance of this office he was closely watched by myself. The ferrules, which were of thick brass, had been rounded, not only to make the contact more visible, but because the length of each rod was determined, by their having the spherical figure, more

easily. At the placing of every *second* rod, which was painted white to distinguish it from the other, I registered its number myself in a book, ruled purposely with columns, each column containing ten numbers: my writer did the same in another book: besides which, an attendant, who was furnished with ten small sticks, gave the Tindal, who also assisted in keeping the reckoning, one of them every time the white rod was laid down; and each man made his separate report to me every tenth number. By these precautions, almost all possibility of a mis-reckoning was prevented; and we accordingly found no disagreement throughout. The whole distance was afterwards re-measured, and gave correctly the same number of rods.

At the conclusion of each several measurement, that I might know exactly where to resume it, a stake, three or four feet long, was driven into the ground, till its head became even with the surface. On the top of this stake the but of the last rod was laid; and a line drawn across it with a pointed instrument, shewed precisely where to re-commence the work. The heads of these stakes served likewise as fixed marks on which, at any time, to place the five smaller flags, or the instrument for taking the requisite angles.

The sum of the six measured lines amounted, by the first trial, to *seven bundred double rods, twenty feet six inches and a half*; and by the second, to *seven bundred double rods, twenty-two feet eleven inches and a half*; their difference being *two feet, four inches and a half*, by which the second measurement exceeded the first. The shortest of these measures is made use of, as operations of this nature have always a tendency to excess, rather than deficiency.

Besides this *linear measurement*, seven essential angles were taken (each angle several times over, for security) with an excellent theodolite by RAMSDEN. These were the angles formed, at each extremity of the base, by the nearest intermediate flag, and the remote signal; and those formed at each intermediate flag, by the nearest flag to it, on each hand.

The nature and purport of all this will be easily understood, by inspecting the accompanying figure, Tab. III. fig. 1. and attending to the following explanation and

DEMONSTRATION.

Let N and S represent the northern and southern signals, placed at each extremity of the base line NS.

Let ABCDE represent the positions of the five intermediate flags, between each of which, including the two signals, the six right lines, before mentioned, were measured.

At each of these points the angle formed by the flag, or signal, on each side of it, was taken with the theodolite: for instance, at A, the angle formed at that point by the northern signal N, and flag B ($177^\circ\ 24'\ 40''$); at B, the angle formed by the flags A and C ($176^\circ\ 23'$); and so on. At each end of the base line, the angle formed by the nearest flag and the distant signal was also taken; *e. g.* at N, the angle SNA, and at S, the angle NSE.

These things being premised, to find the angles the other four lines make with the base line, proceed in the following manner:—Produce the line NA to K; also the line BA to H; and, through the point A, draw FG parallel to NS, the base line. From the points ABCDE let fall perpendiculars upon the base line, meeting it at OPQR and T.

The angles SNA, NAF, GAK, are all equal; for SNA and NAF are alternate angles, formed by a right line falling upon two parallels (Euclid, 29th, 1st): and NAF, GAK, are equal angles, formed by the mutual intersection of two right lines (Euclid, 15th, 1st). For the same reason, the angle NAH is equal to KAB, and HAF to GAB. Now, the obtuse angle NAB, being known (from observation), its supplement is equal to BAK, which, being subtracted from GAK, leaves the angle GAB required; by means of which, and the hypothenuse AB (given by mensuration), we obtain the side AG of the right-angled triangle GAB, by plane trigonometry; for as rad.: cosine ∠GAB : : side AB : side AG. This computed side is equal to the opposite side OP of the rectangular parallelogram OPAG, which being added to the side NO of the right-angled triangle ONA (found by the like analogy from the observed angle N, and the measured hypothenuse NA) gives the distance NP on the base line.

Proceeding in like manner, with the last found angle, to find the angles of the remaining triangles, and thence their respective bases, we shall have the sides of the corresponding rectangles; the sum of which sides, taken together, will be the true measure of the base line NS.

Supposing no imperfection in the instrument with which the angles have been taken, and no error (however small) committed in taking the six preceding angles, then the angle XES, obtained by this method, will prove exactly equal to the angle NSE, which may be had by observation. In the present instance their difference amounts to no more than $2' 5''$.

In the example before us, the observed angles are as follow, from which we may easily deduce the remaining ones required:

Angle at N	5° 36′ 30″	Angle at D	178° 37′ 30″
∠ A	177 24 40	∠ E	177 2 0
∠ B	176 23 0	∠ S	6 53 0
∠ C	178 1 15		

With which proceed thus:

NAB 177° 24′ 40″

Its supplement — 2 35 20
from 5 36 30 ∠ N

gives 3 1 10 ∠ required at A.

ABC 176° 23′ 0″

3 37 0
— 3 1 10

0 35 50 ∠ required at B.

BCD 178° 1′ 15″

1 58 45
+ 0 35 50

∠ required at C 2 34 35

CDE 178° 37′ 30″

1 22 30
+ 2 34 15

∠ required at D 3 57 5

DES 177° 2′ 0″

2 58 0
+ 3 57 5

6 55 5 ∠ req. at N.

E or S. Diff. only 2′ 5″ from the observed angle.

It is immaterial at which end of the base line we begin. In the present case, in order to obtain as great precision as possible, the intermediate angles have been deduced from both ends; those at A and B, from the observation at N; and the remaining ones at C D and E, from that at S. Had, however, this precaution been neglected, the error induced, by deriving the four required angles from either primitive, at N or S, would not have affected the true length of the whole base line more than 0,27 of a foot, or not quite so much as three inches and a quarter.

This method of obtaining the measure of an inaccessible line, where the measured lines every where make small angles with it, is a very accurate one; for though, in oblique triangles, small angles, from the difficulty of taking angles perfectly, are likely to produce considerable errors, in right-angled triangles it is the very reverse; as in them the smaller the angle taken, the more accurate will be the result.

Fig. 1. is not drawn in due proportion, either as to the sides or angles; as, had it been justly delineated, there would not have been sufficient room for the auxiliary lines, and letters of reference. Fig. 2. is protracted by scale and sector, and shews the relative length and bearing of the several measured lines, as well as the direct distance of their farthest extremities, and their position with respect to the meridian.

For distinction sake, the measured hypothenusal lines are drawn thicker than those about them; and the lines which are parallels to the base line are dotted.

Table of the measures, with their corrections, of the six hypothenusal lines.

Note, the error of the rod A was 0,10 Inch. +, of B 0,12 Inch. +.

Distances and number of double rods between the stations.	Measured lengths in feet, &c.			Corrections; subtractive.			Correct length of each hypothenuse.		
D. Rods. Feet. Inches.	Feet.	Inch.	Dec.	Feet.	Inch.	Dec.	Feet.	Inch.	Dec.
Between N signal and 1st flag 146 9 4	7309	4	0	2	8	1	7306	7	9
Between 1st and 2d flags 170 37 6	8537	6	0	3	1	5	8534	4	5
Between 2d and 3d flags 106 11 6	5311	6	0	1	11	4	5309	6	6
Between 3d and 4th flags 83 10 3	4160	3	0	1	6	4	4158	8	6
Between 4th and 5th flags 93 8 2	4658	2	0	1	8	5	4656	5	5
Between 5th flag and south. signal 100 48 5,7	5043	5	7	1	10	2	5041	7	5
				Total effect of error, 12 10 1+					

Table for computing the R. A. Triangles, and reducing the six measured lines to one rectilinear distance.

Angular points.	Angles.	Correct Hypothenuses.	Logarithms.	Reductions.
At north. signal N	5° 36′ 30″ W^{ly}.	1st Hypothenuse, Feet. Dec. 7306, 66 NA	9. 9979161 3. 8637189 3. 8616350	Feet. Dec. 7271 68 NO
At 1st flag A	3 1 10 W^{ly}.	2d Hypothenuse, 8534, 38 AB	9. 9993967 3. 9311720 3. 9305687	8522 53 OP
At 2d flag B	0 35 30 E^{ly}.	3d Hypothenuse, 5309, 55 BC	9. 9999764 3. 7250577 3. 7250341	5309 26 PQ
At 3d flag C	2 32 30 E^{ly}.	4th Hypothenuse, 4158, 72 CD	9. 9995725 3. 6189596 3. 6185321	4154 63 QR
At 4th flag D	3 55 0 E^{ly}.	5th Hypothenuse, 4656, 46 DE	9. 9989845 3. 6680559 3. 6670404	4645 59 RT
At 5th flag. E and south. sig. S	6 53 0 E^{ly}.	6th Hypothenuse, 5041, 63 ES	9. 9968584 3. 7025710 3. 6994294	5005 29 TS 34908 98 NS

Miles. F. Yards.
Or 11636 English yards (rejecting the Decimals), or 6 4 196 total length of the *base*.

Observations taken, both with the theodolite, and the Hadley, for determining the position of the base line, with respect to the meridian.

1788	App. time.	Distance of ⊙'s centre from signal observed.	⊙'s altitude, centre cor.	⊙'s azimuth computed.	Horizontal ∠ between ⊙'s centre and signal.	Angle made by signal with meridian.
May	H. ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
					With theodolite, mean of 6 sights,	
24	about 7 A. M.	—	21 52 50	71 59 18	111 34 0	3 33 18
	about				mean of 6	
25	7 A. M.	—	19 22 37	71 32 1	68 3 35	3 28 26
	about				mean of 6	
27	5 P. M.	—	18 45 54	71 3 40	105 26 35	3 29 46
	about				mean of 6	
29	7½ A. M.	—	29 20 18	71 29 20	111 56 8	3 25 28
	about				mean of 6	
29	4½ P. M.	—	27 45 41	71 21 34	105 9 28	3 29 6
					mean	3 29 12
		mean of 4				
	A. M.	Hadley obs.	computed		computed	
29	7 18 26	110 35 30	22 19 26	71 6 30	112 21 10	3 27 40
	P. M.	mean of 3	computed		computed	
29	4 27 39	103 42 9	25 35 0	71 17 40	105 13 47	3 28 33
					mean	3 28 6

The mean of 28 observations by the theodolite is - 3° 29′ 12″
The mean of 7 observations with the Hadley is - - 3 28 6
The medium of both is therefore (by which the south end of the base is westerly, and the north end easterly of the meridian) - - - 3 28 39

N. B. The set of observations, taken as above, on the 25th of May, were made at the southern signal; all the rest at the northern signal.

Meridional observations of stars for determining the latitude of each end of the base line.

Stars.	True zenith at south end of base.	Distances at next southern station.	Latitude of south end of base.
β in Centaur -	70° 54′ 8″	—	11° 33′ 34″
α in Centaur -	71 31 19	—	11 33 48
Antares - -	37 30 14	—	11 33 40
α in Centaur -	—	71 27 4	11 33 34
β in Cross - -	—	70 0 50	11 33 2
η in Ursa Major	—	38 53 16	11 33 14
β in Centaur -	—	70 49 11	11 32 43
Antares - -	—	37 25 58	11 33 25
η in Ursa Major	—	38 53 1	11 33 29
β in Centaur -	—	70 50 11	11 33 43
α in Centaur -	—	71 26 58	11 33 27
η in Ursa Major	—	38 53 16	11 33 14
β in Centaur -	—	70 49 26	11 32 58
Mean latitude of south end -			11 33 22
		+	5 42
Latitude of north end of base line			11 39 4

To settle the position of the base, with respect to the meridian, the sun's bearing from each signal, and his azimuth, were observed several times, both with the theodolite and the Hadley; and, having made the necessary computations, I became satisfied of the justness of an opinion I had before entertained, that the Hadley is by far the best instrument in general practice for such purposes: for though the theodolite has the advantage, from its fixed position, and

the power of its telescope, in taking horizontal angles *upon the horizon*; yet, at any considerable elevation, when a strict attention is required to the vertical adjustments of the theodolite, such attention is incompatible with the nature of a portable instrument, which is ever liable to suffer change in its adjustments by even the most careful removal from place to place.

The table, page 110, gives the results of these observations, and the data from which they were computed. Nothing need be said of the two preceding tables, which sufficiently explain themselves.

The last table contains meridional observations of stars for determining the latitude of each respective termination of the base line. No observations were obtained at the northern extremity, the weather proving uncommonly cloudy while I was there; and only three latitudes were observed at the south end. The others were taken at my next southern station, and reduced to the terms of the base by the adjacent triangles. The difference of latitude between that station and the south end of the base, I found by measurement to be 8201 yards, or 4′ 1″; and the difference of latitude between the terms of the base 5′ 42″. For considering a portion of the meridian passing through either end of the base, as the cosine of an arc, whose radius is 11636 yards, and subtense 3° 28′ 39″, we shall find the difference of latitude to be 11614 yards = 5′ 42″. By a mean of all the astronomical observations, the south end of the base lies in latitude 11° 33′ 22″ N.; the north end consequently lies in latitude 11° 39′ 4″.

Having completed the measurement and principal calcu-

lations, I caused a large stone to be placed at each extremity of the base, to mark and perpetuate it for future occasions. The stone at the north end I inscribed with the following characters:

S. 3° 29′ W.
Yds.
11636.
T.
1788.

And that at the south end as follows:

N. 3° 29′. E
Yds.
11636.
T.
1788.

both inscriptions equally implying, that the opposite end of the base line lies in the direction therein expressed, distant 11636 English yards from the stone so inscribed.

It may be proper to record, that the north end of the base is on the sea beach, 27 yards from the surf; and 129 yards N. E. by E. easterly, from an old choultry in a Palmyra tope, called Namaswaumoodaly Choultry. From the mouth of Cuddalore river to the north end of the base I measured, with a perambulator, just four miles and one furlong.

The south end of the base lies 42 yards from the surf, and 169 yards E. $\frac{1}{2}$ S. from a decayed choultry in a Palmyra tope, called Chinnachitty Choultry. There is another choultry and tope near the centre of the base; but the northern cluster

of trees before mentioned, is the first cluster we meet with, by the sea side, southward of Cuddalore river.

This measured base makes the exterior side of the thirty-third triangle of a concatenated series of oblique triangles, now carrying down the coast from the steeple of Fort Saint George.

Porto Novo,
10th June, 1788.

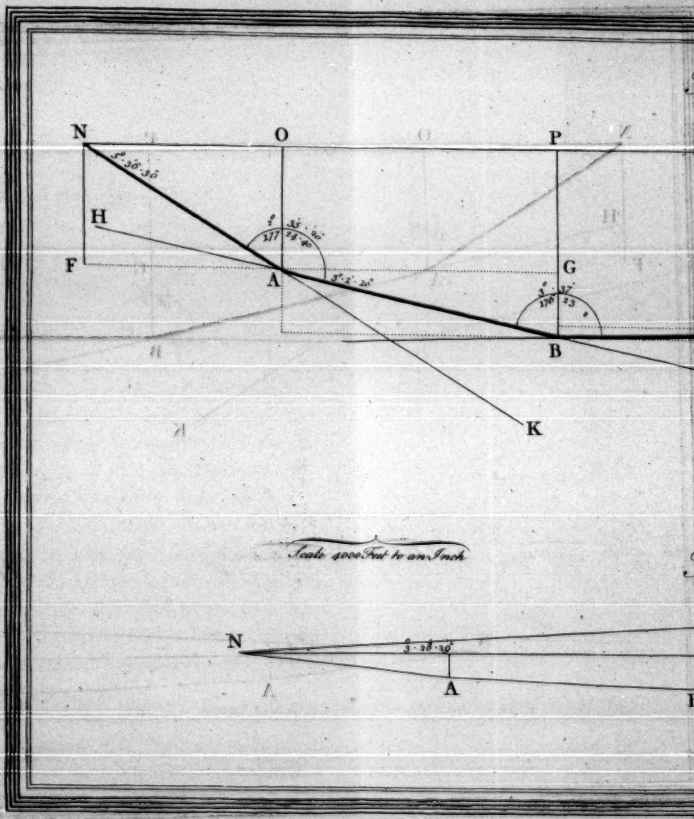

N
O
P
H
F
A
G
B
K
Scale 3000 Feet to an Inch.
N
A

Fig. 1.

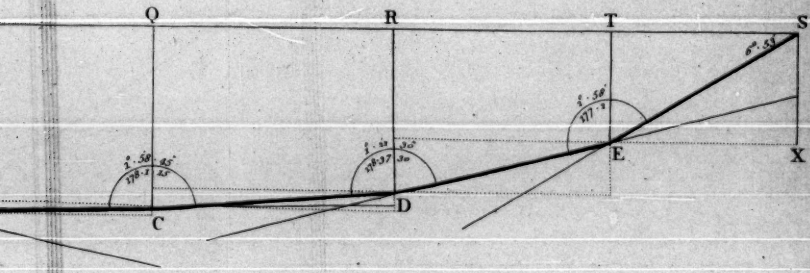
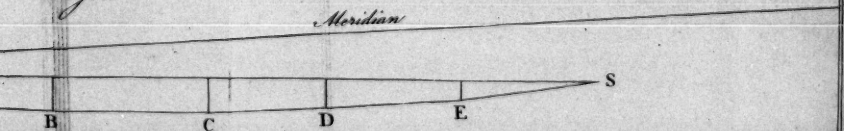


Fig. 2.



VII. *Description of* Kilburn Wells, *and Analysis of their Water.* *By Mr.* Joh. Godfr. Schmeisser. *Communicated by Sir* Joseph Banks, *Bart. P.R.S.*

Read February 23, 1792.

THOSE wells lie to the right of the Edgeware Road, about two miles from London, in a dry, but verdant, and gently rising meadow.

They spring about 12 feet below the surface, and are covered with a small stone cupola.

The diameter of the well near the surface of the water is about five feet; the depth of the water was in July and August two feet; this its general depth increases in winter, at times, to three feet; the changes in the atmosphere do not appear to affect either the quantity or quality of the water.

This mineral water is not perfectly bright, but of rather a milky hue; it has a mild and bitterish taste, with little or no briskness, as containing a very small proportion of fixed air. On dipping for the water, or otherwise agitating it, a sulphureous smell is perceived near the surface; which, however, soon goes off in a temperature of about 80 degrees of FAHRENHEIT's thermometer, *which I have used throughout*.

The specific gravity of the Kilburn water is to distilled water as 1,0071 : 1,0000; its general temperature 55°, which was not affected by a change of 10 degrees in the temperature of

the atmosphere. While the water continues at rest, no bullition of fixed air is perceived, and scarce any sulphureous smell.

That this mineral water so easily parts with the hepatic air (perceivable on agitating it) if it be shaken in a warmer temperature, or transported from one place to another, is probably owing to the fixed air which it contains: for as this aerial acid has a great affinity to phlogiston; so it may hence be inferred, that fixed and hepatic air cannot exist together in a mineral water, but that the latter will be destroyed, as the fixed air is developed by gentle warmth.

Chemical Experiments. Examination of the Kilburn *Waters by reagent Substances.*

EXPERIMENT I.

The tincture of litmus is a very little affected by the water fresh from the spring, and not at all after having been boiled; which proves that this water contains very little aerial acid.

EXPERIMENT II.

Paper stained with a decoction of logwood is somewhat changed, to rather a bluish hue, by fresh water; from this I infer, that the water contains a little absorbent earth dissolved in aerial acid.

EXPERIMENT III.

Paper stained with turmeric is not changed by this water; which would happen if it contained any uncombined alkaline salt.

EXPERIMENT IV.

On adding 42 drops of the purest concentrated vitriolic acid to two pounds of the Kilburn water, it became perfectly clear, and some air was disengaged; this air rendered lime-water turbid.

EXPERIMENT.—A few drops of pure and nitrous acid were dropped into a tumbler full of the water; the smell of hepatic air was diminished, and hardly any precipitate formed. From this experiment it becomes probable, that the water contains no liver of sulphur, but only hepatic air: from the appearances on adding the vitriolic acid, may be inferred, that this water contains little calcareous earth, and no *terra ponderosa*.

EXPERIMENT V.

In order to ascertain whether the hepatic air really existed in the water, or whether the appearances which made this probable might not arise from the air of marshes, which will occasionally imitate the other, I filled three quart bottles with distilled water, and nearly emptied them just over and almost in contact with the spring. The air, which of course took the place of the water I had emptied, was subjected to the following experiments:

(a) A piece of white arsenic being immersed in it, its surface soon became yellow.

(b) A solution of lead being put into one of these bottles, the precipitate which was formed soon became of a blackish brown colour.

(c) A solution of silver being put into the third bottle, the precipitate formed was blackish. All these are proofs of the existence of the hepatic air.

EXPERIMENT VI.

On adding a few drops, both of the aqueous and spirituous infusion of galls, to a glass of the fresh water, no change of colour took place; yet by means of well saturated phlogisticated alkali some traces of iron were perceived.

EXPERIMENT VII.

A solution of soap in spirit of wine being dropped into the water, was immediately decomposed by it. This proves it to contain neutral salts.

EXPERIMENT VIII.

By adding some acid of sugar both to the fresh and the boiled Kilburn water, calcareous earth was precipitated; which shows that this water contains aerated calcareous earth, and selenite.

EXPERIMENT IX.

By adding aerated volatile alkali, magnesia and calcareous earth were precipitated both from the fresh and the boiled water.

EXPERIMENT X.

Caustic volatile alkali precipitated both magnesia and calcareous earth from the fresh water; a proof of its containing these earths in a state of combination with the aerial and other acids.

EXPERIMENT XI.

Caustic fixed alkali precipitated from the fresh water aerated magnesia. (It is to be observed with regard to this

experiment, that the precipitate should be separated as soon as formed; otherwise, if there be iron contained, that will also be precipitated together with calcareous earth).

EXPERIMENT XII.

On dropping muriated *terra ponderosa* into the fresh water, the earth is precipitated; which also happens, but in a less degree, if the water has been boiled; this proves it to contain vitriolated soda and magnesia, the other selenite.

To ascertain the quantity of vitriolic acid contained in these salts, as much pure acetous acid was first added to one pound of the water, as was required to saturate the earth. Then a solution of *terra ponderosa* in nitrous acid was carefully dropped into the mixture, till no more precipitate was formed; the thus regenerated spar was carefully collected, edulcorated, and dried, when it weighed 60 grains. Now, if 100 grains of ponderous spar contain 22 grains of vitriolic acid, it will follow, that one pound of the Kilburn water contains about 13 grains of this acid.

EXPERIMENT XIII.

Vitriolated silver was dissolved, and added to the Kilburn water, previously impregnated with pure nitrous acid, to effect a solution of the earthy particles contained in it. The silver combined with the muriatic acid in the water, and formed a *luna cornea*: but I do not estimate the quantity of the acid in the water from this experiment, which is liable to deceive, as well as the preceding. This will appear on comparing the result with the real quantity of vitriolic acid, as given in the contents of the water, annexed to these experiments.

EXPERIMENT XIV.

A quantity of the Kilburn water having been gently evaporated to dryness, a powder remained; some of this being triturated with vegetable alkali, there was no smell of volatile alkali perceived.

EXPERIMENT XV.

A little of the powder having been mixed with tartar, and thrown into a red hot crucible, no detonation happened; of course nitre was not one of the constituent parts.

EXPERIMENT XVI.

On moistening a little of the powder with pure and concentrated vitriolic acid, there arose muriatic vapours; a proof there were no salts formed with the nitrous acid existing in this water.

The Kilburn water therefore contains fixed air, hepatic air, earthy neutral salts, vitriolated and muriated neutral salts, calcareous earth, magnesia, selenite, and a very little iron.

These component parts of this mineral water appeared on the addition of reacting substances; and with this guide I proceed to the analysis. I beg, however, first to mention another experiment or two, relative to the effects of this water. Two quarts of the fresh drawn water having been successively drank, operated gently downwards, but at the same time affected the head a little. This species of intoxication was, however, not produced, if the water had been freed from its hepatic air. The celerity of the pulse was but little increased by it; yet the following experiment will prove that it pervaded the whole system, and afford a very strong argument in favour of its

efficacy. A small plate of silver was placed under the arm, in contact with the skin, and thus worn for some hours without being tarnished by the perspirable matter; a bottle of the Kilburn water having now been drank, in less than half an hour the silver was become black.

One ounce of fresh gall was mixed with a quart of the water as it came from the spring, and into another bottle was put the same quantity of gall, with a quart of distilled water, and both were placed in a warmth of 96°. After ten hours, the latter mixture began to show signs of putrefaction, while that with the Kilburn water continued perfectly sweet. Twelve hours after, this also became putrid.

Two ounces of very putrid gall were mixed with a quart of Kilburn water, and placed in the same warmth. The fœtor was soon diminished, and after three hours no longer perceptible.

Two ounces of putrid gall were mixed with two ounces of distilled water, in which 24 grains of the saline mass, obtained by evaporation from the Kilburn water, had been previously dissolved; and this mixture was likewise placed in a warmth equal to 96°. After 2½ hours the offensive smell had gone completely off.

Similar experiments were made with blood, and the results were the same.

Experiments to ascertain the Properties and Proportion of the Elastic Fluids, contained in the Kilburn *Water.*

The apparatus with which these experiments were instituted contained 16 cubic inches: the cylinder for the reception of the extracted air, eight cubic inches. Into the jar were put 14

cubic inches of the fresh drawn water; and having been immediately connected with the apparatus, it was placed in a lamp furnace. The heat having been gradually increased till the water began to boil, six cubic inches of the quicksilver, with which the cylinder had been previously filled, were displaced by the air which came over. This vessel having been artificially cooled to 53°, the air was contracted one-fourth of an inch. Now, if the two cubic inches of atmospheric air, left in the jar, be deducted, there remain $3\frac{3}{4}$ cubic inches of air expelled from 14 inches of the water. By agitation in lime-water $2\frac{1}{4}$ cubic inches were absorbed, the precipitated aerated lime weighed $2\frac{1}{8}$ grains. The remaining gas was found to be hepatic air.

Experiments to ascertain the fixed constituent Parts of the Kilburn *Water, and their Properties.*

EXPERIMENT I.

Twenty-four pounds of water (at 16 ounces) were evaporated in one of the Wedgwood basons, by a gentle heat, down to four ounces; this residuum was then reduced to perfect dryness in a small glass vessel. The mass thus obtained was scaly, with crystals intermixed, and of a yellowish hue; its taste was bitter, and but little sharp; the weight $1900\frac{999}{997}$ grains (which were equal to 1560 grains of the further used accurate weight), which divided by 24, gives for every pound of the water 79 grains of solid matter. The bason in which the first evaporation had been made, was rinced with a little *aqua regia*, that such earthy particles as might have adhered to it should not be lost; this solution was put aside and marked A.

EXPERIMENT II.

The dry residuum obtained as above was rubbed with a little alcohol, and a sufficiency of this spirit having been added, the whole was placed in a gentle warmth, and often stirred with a glass tube: after a few days the fluid was decanted, and what remained indissoluble, having been edulcorated with alcohol, and carefully collected and dried in a moderate warmth, was found to weigh 1392 grains; so that 168 grains had been taken up by the alcohol.

The spirituous solution was gently evaporated, when 180 grains of a yellowish, easily deliquescent salt remained, having a bitter and acrid taste. By adding to this one ounce of the strongest alcohol, all the deliquescent salts were dissolved, leaving 40 grains of a saline substance; which having been again dissolved in distilled water and crystallized, was found to be common salt.

The solution of the deliquescent salts having been mixed with 20 drops of pure vitriolic acid, some selenite appeared; the whole was now evaporated to dryness, and having been mixed with $\frac{1}{5}$ of its weight of pure vitriolic acid, it was exposed to a considerable heat. The vapours thus expelled were those of the muriatic acid. When these had ceased, and the mass was cold, I again dissolved it in distilled water, when a black flaky substance was separated, which being carefully collected on filtering paper, and dried, weighed six grains; this was resinous matter.

The solution was now placed on the fire, in order to evaporation, during which 12 grains of selenite were separated; the remainder afforded vitriolated magnesia, leaving some drops of a yellowish fluid, from which, by the addition of caustic

volatile alkali, a very small quantity of calx of iron was precipitated, which, when dry, weighed about $\frac{1}{8}$ of a grain.

It appears, from the above experiments, that the spirituous solution held of

Muriated soda - -	40 grains.
——— magnesia - -	128 grains.
——— calcareous earth -	6 grains.
Resinous matter - -	6 grains.
Calx of iron - -	$\frac{1}{8}$ grain.

EXPERIMENT III.

The residuum of the second experiment, which was not soluble in alcohol, was digested with distilled water, and often stirred. The water took up 1204 grains. This solution was filtered, the residuum often edulcorated and dried; this weighed 188 grains. The solution was evaporated in a gentle warmth to one third, and being then set in a cold place, 12 grains of selenite were separated. Having further evaporated the remaining solution, I now mixed it with double its weight of alcohol, and after having again heated this mixture, I let it cool gradually; thus all the vitriolic salts were separated. I again dissolved this saline mass in distilled water, and, after gentle evaporation, obtained crystals, weighing altogether 1200 grains, and consisting of vitriolated tartar, and vitriolated soda: from the remaining ley I obtained, on further evaporation, 10 grains more of common salt. There were no traces of an uncombined alkali, which must otherwise have now shewn itself. The 1200 grains of mixed salts, which had crystallized first, were again dissolved in water, and this solution made to boil; a hot solution of mineral alkali was now mixed

with it, and the magnesia thus separated weighed, when washed and dried, 170 grains: this having been again saturated with diluted vitriolic acid, afforded 910 grains of pure crystallized vitriolated magnesia. The solution remaining after the separation of the magnesia having been duly evaporated, yielded 282 grains of crystallized GLAUBER salts, exclusive of what had been formed by the above-mentioned admixture of the natron.

EXPERIMENT IV.

The 188 grains of remaining earth, mentioned in the preceding experiment, were put into *aqua regia*, and the solution mentioned in the first experiment, as marked (A), was added. This mixture having been well heated, and again suffered to cool, was put on some filtering paper; and what remained on this, having been well washed with diluted spirit of wine, and dried, weighed 112 grains.

The filtered solution was then gently evaporated, during which it deposited six grains of selenite; a sufficiency of phlogisticated alkali was now added, to separate the iron; 25 grains were required, and the dried blue precipitate weighed 15 grains. (It is to be observed, that the phlogisticated alkali contained, in 25 grains, $4\frac{1}{2}$ grains of the calx of iron.)

The above blue precipitate was put into a small crucible, and kept for a proper time in a red heat, when it left $7\frac{1}{2}$ grains of calx of iron, which was attracted by the magnet; if from this be deducted the $4\frac{1}{2}$ grains contained in the alkali, there will remain three grains which had been contained in the water.

EXPERIMENT V.

The solution, from which I had precipitated the iron, was evaporated, and mixed with vitriolic acid, and diluted spirit of wine, when 72 grains of selenite were separated: these 72 grains of selenite having been boiled with mineral alkali, yielded 24 grains of calcareous earth.

EXPERIMENT VI.

The remainder of the solution from which the calcareous earth had, by means of the vitriolic acid, been separated in the form of selenite, yielded, by adding mineral alkali, $72\frac{1}{2}$ grains more of magnesia. All the selenite obtained was boiled in 1200 times its weight of water, in which it was completely dissolved, no siliceous earth being left.

As a proof that the process had been properly conducted, I saturated both the obtained earths with diluted vitriolic acid; when the first again afforded selenite, and the other vitriolated magnesia.

Summary of the constituent parts of the Kilburn *Water, in* 24 *Pounds.*

Fixed air - -	84	cubic inches	
Hepatic air - near	36		
Vitriolated magnesia -	910	grains, equal to	℥ij ʒiiss, Apothecary's weight.
Vitriolated natron -	282	gr.	= ʒv. lij grains
Muriated natron -	60	gr.	= 75 gr.
Selenite - - -	130	gr.	= ʒij xlij gr.
Muriated magnesia -	128	gr.	= ʒij xl gr.
——— calcareous earth	6	gr.	= $7\frac{1}{2}$ gr.
Aerated magnesia -	$12\frac{1}{2}$	gr.	= 15 gr.
——— calcareous earth	24	gr.	= 80 gr.
Calx of iron -	$3\frac{1}{8}$	gr.	= 4 gr.
Resinous matter -	6	gr.	= $7\frac{1}{2}$ gr.

Sum $1561\frac{1}{8}$ grains, equal to medicinal weight 4 ounces, 0 drams, and 32 grains.

VIII. *Observations on Bees.* *By* John Hunter, *Esq. F.R.S.*

Read February 23, 1792.

Of the Common Bee.

The common bee, from a number of peculiarities in its œconomy, has called forth the attention of the curious; and, from the profit arising from its labours, it has become the object of the interested; therefore, no wonder it has excited universal attention, even from the savage to the most civilized people: but it has hardly been considered by the anatomist; at least the two modes of investigation have not gone so much hand in hand, as they ought to have done.

The history of the bee has rather been considered as a fit subject for the curious at large, whence more has been conceived, than observed. Swammerdam, indeed, has rather erred on the other side, having, with great industry, been very minute on the particular structure of the bee. I shall here observe, that it is commonly not only unnecessary to be minute in our description of parts in natural history, but in general improper. It is unnecessary, when it does not apply to any thing, but the thing itself, more especially if it be of no consequence; but whenever it applies, then it should so far be treated accurately. Minutiæ beyond what is essential, tire the mind, and render that which should entertain along with

instruction, heavy and disagreeable; the more so too, if the parts are small, where the sense can only take them in singly, and the mind can hardly comprehend the whole, or apply all the parts combined to any consequent action. This has been too much the case with SWAMMERDAM; he often attempted too much accuracy in his description of minute things. But the natural history of insects has not been sufficiently understood at large, so as to throw light on this subject where there was an analogy, and where, without such analogy, it must appear in the bee alone unintelligible, from the obscurity attending some parts of their œconomy; for there is hardly any species of animals but what has some of its œconomy obscure; and probably this is as much so in this insect, as in any other class of animals we are at one season of the year almost daily seeing; yet these parts of the œconomy may be evident in some other species of the same tribe or genus, and thus be cleared up, from analogy, so that the species assist each other in their demonstration. This is evident in the whole tribe of flying insects, for what is lost, or cannot be made out in the one, may be demonstrated in another: and we find there are some things in the œconomy of the bee that cannot be seen or demonstrated in it alone, but which are evident in some other insects; and while they possess the same parts, and other circumstances are similar, we must conclude the uses of those parts are similar in both; for whenever a circumstance in one animal cannot be found out in that animal, but can in another, then the natural conclusion is, that the uses are similar in both.

Though the bee may be classed in some degree among the domestic animals, yet from there being such a cluster

of them, and because they are an offensive and irritable animal, their actions are rendered very obscure, and can only be observed by little starts; often we can only see the effects, which renders the knowledge of their œconomy still imperfect; they would in many cases seem to evade our wishes; they often remove out of our sight part of their œconomy, when they can. Thus they often remove their eggs and young. Many quadrupeds do this, as cats, &c. and I have reason to believe, that birds can remove their eggs, at least I have reason to suspect the sparrow of this.

As the bee is an insect, it has most things peculiar to that class of animals: such as are common are not to be taken notice of in the history of this insect, but only its peculiarities which distinguish it from all others, and constitute it to be a bee; and as bees form a large tribe of insects, it is the more singular peculiarities that constitute a distinct species of this tribe. As most parts of the œconomy of insects have not been in every respect understood, and although now known in some insects, yet cannot be observed in the bee, but which accord with many circumstances attending this insect, therefore such must be brought into the present history of the bee, to render it more complete. I shall not be minute in the anatomy of this animal, as that would be too tedious and uninteresting. When we talk of the œconomy of the colony, such as the secreting wax, making combs, collecting farina, honey, feeding the maggots, covering in the chrysalis, and the honey, stinging, &c.; it is the labouring bees that are meant. In pursuing any subject, most things come to light as it were by accident; that is, many things arise out of investigation that were not at first conceived, and even

misfortunes in experiments have brought things to our knowledge that were not, and probably could not have been previously conceived: on the other hand, I have often devised experiments by the fireside, or in my carriage, and have also conceived the result; but when I tried the experiment, the result was different; or I found that the experiment could not be attended with all the circumstances that were suggested.

As bees, from their numbers, hide very much their operations, it is necessary to have such contrivances as will explore their œconomy. Hives, with glass lights in them, often shew some of their operations, and when wholly of glass, still more; but as they form such a cluster, and begin their comb in the centre, little can be seen till their work becomes enlarged, and, by that time, they have produced a much larger quantity of bees, so as still to obscure their progress. Very thin glass hives are the best calculated for exposing their operations; the distance from side to side about three inches; of a height and length sufficient for a swarm of bees to complete one summer's work in. As one perpendicular comb, the whole length and height of the hive, in the centre, dividing it into two, is the best position for exposing their operations, it is necessary to give them a lead or direction to form it so; therefore it is proper to make a ridge along the top from end to end, in the centre, between the two sides, for they like to begin their comb from an eminence; if we wished to have them transverse, or oblique, it would only be necessary to make transverse, or oblique ridges in the hive. I had one made of two broad pieces of plate-glass, with glass ends, which answered for simple exposure very well; but I often saw operations going on, when I wished to have caught some of the bees, or to take out a piece of comb,

&c.; therefore I had hives made of the same shape and size, but with different panes of glass, each pane opening with hinges, so that if I saw any thing going on that I wished to examine more minutely or immediately, I opened the pane at this part, and executed what I wished, as much as was in my power; this I was obliged to do with great caution, as often the comb was fastened to the glass at this part. When I saw some operations going on, the dates or periods of which I wished to ascertain, such as the time of laying eggs, of hatching, &c. I made a little dot with white paint opposite to the cell where the egg was laid, and put down the date.

From these animals forming colonies, and from a vast variety of effects being produced, and with a degree of attention and nicety, that seem even to vie with man; man, not being in the least jealous, has wished to bestow on them more than they possess, viz. a reasoning faculty; while every action is only instinctive, and what they cannot avoid or alter, except from necessity, not from fancy. They have been supposed to be legislators, even mathematicians: indeed, upon a superficial view, there is some shew of reason for such suppositions; but people have gone much further, and have filled up from their imagination every blank, but in so unnatural a way, that one reads it, as if it were the description of a monster. Probably, the best way of treating the history of this insect, is only to describe what is, and the reader will immediately see where authors have been inventing; however, there are some assertions that should be particularly taken notice of, such as forming queen bees at pleasure.

Countries that have but little variety in their seasons may have insects, whose œconomy is well adapted to this uniformity,

and which would not be suited to a climate whose seasons are very different; for insects of countries, whose seasons are strongly marked, as in this, have a period in their life which it is little in our power to investigate, and can scarcely be discovered but by accident, for experiments often give little assistance; therefore we are obliged to fill up this blank by reasoning, and from analogy, where we have any. This period is principally the winter, in those insects who live through that season. Animals of season are somewhat like most vegetables; while the common bee is only an animal of seasons in the common actions of life, or what may be called its voluntary actions, and therefore is somewhat like the human species, suited to every country; which may be the reason why it is so universal an animal, for I believe bees are one of the most universal animals known: yet this may arise from cultivation, in consequence of which, they have been brought into climates, where, of themselves, they would not have come.

Insects are so small, and so few of them are capable of being domesticated, that the duration of their life is not easily ascertained; therefore we are to rely more on circumstantial, than on positive or demonstrative proof; and perhaps the life of the common bee may be least in our power to know, for their numbers in the same society make it almost impossible to be ascertained. From their forming a colony, or society, which keeps stationary, the continuance of this society is known, but to what age the individual lives, is not known; we are certain, however, that it is only the labourers and queens that continue the society, for the males die the same year they are formed. From their fixing on the branches of

trees, under projecting exposed surfaces, when they swarm, we should be inclined to suppose that they were animals of a warm climate; yet their providing liberally for the change of climate, or rather for a change of season, would, on the contrary, make us believe they were adapted for changeable climates; or rather, these two circumstances should make us suppose they were fitted for both; and their universality proves it. And I do conceive, that in a pretty uniform warm climate, their œconomy may be somewhat different from what it is in the changeable, as they would not be under the same necessity to lay up so much store, and probably might employ their cells in breeding, for a much longer period: however, a good climate agrees with them best, as also a good season in an indifferent climate, such as Britain. We find the common bee in Europe, Asia, Africa, and America. That they may be, or should be in the three first, is easily supposed, but how they came to America is not so readily conceived; for although a kind of manageable animal, yet they do not like such long confinement in their hives, as would carry them to the West Indies, excepting in an ice-house; for when I have endeavoured to confine them in their hives, they have been so restless as to destroy themselves.

The female and the working bee, I believe, in every species have stings, which renders them an animal of offence, indeed, but rather of defence; for although they make an attack, I believe it is by way of defence, excepting when they attack one another, which is seldom or never with their stings. As this belongs more to the labourers, it shall be considered when I treat of them in particular. Of the whole bee tribe, the common bee is the easiest irritated; for as they have property,

they are jealous of it, and seem to defend it; but when not near it, they are quiet, and must be hurt before they will sting; with all this disposition for defence, which is only to secure their property, or themselves, when more closely attacked, yet they have no covetousness, nor a disposition to obstruct others. Thus two bees or more will be sucking at the same flower, without the first possessor claiming it as his right: a hundred may be about the same drop of honey, if it is beyond the boundaries of their own right; but what they have collected they defend. It is easily known when they mean to sting; they fly about the object of their anger very quickly, and by the quickness of their motion evade being struck or attacked; which is discovered by the sound of their wings, as if going to give a stroke as they fly, a very different noise from that of the wings when coming home of a fine evening loaded with farina, or honey; it is then a soft contented noise. When a single bee is attacked by several others, it seems the most passive animal possible, making no resistance, and even hardly seeming to wish to get away; and in this manner they allow themselves to be killed. They are perhaps the only insect that feeds in the winter, and therefore the only one that lays up external store; and as all animals, whether insects or not, that keep quiet in the winter, without either eating at all, or eating very little in proportion to what they do in the summer, grow fat and muscular in the summer, (which I term internal store,) we see why the common bee need not be fatter at one time than another; and accordingly we find them nearly of the same fatness the year round.

There are accidents befalling hives of bees, that are not easily accounted for. I had a hive which in the month of November

was become quite empty of bees, and upon examination had no honey in it, which was strong in the summer, and had violent attacks made upon it in October by wasps belonging to a nest in the garden, but appeared quiet when that nest was removed. Upon examining this hive, I found only five dead bees, and not a drop of honey in any one cell: there was a good deal of bee bread in different cells scattered up and down the comb, which was become white with mould on its surface. On the other hand, I have had swarms die in the winter in the hives, while there was great plenty of honey in the combs: what seemed remarkable, they all died with their probosces elongated, and in those which I opened, I found the stomachs full of honey, and their intestines full also of excrement, especially the last part.

Of the Heat of Bees.

Bees are, perhaps, the only insect that produces heat within itself, and were therefore intended to have a tolerably well-regulated warmth, without which, of course, they are very uncomfortable, and soon die; and which makes not only a part of their internal œconomy respecting the individual, but a part of their external, or common œconomy, and is therefore necessary to be known. The heat of bees is ascertainable by the thermometer, and I shall give the result of experiments made at two different seasons of the year.

July 18th, at ten in the evening, wind northerly, thermometer at 54°, in the open air, I introduced it into the top of a hive full of bees, and in less than five minutes it rose to 82°. I let it stand all night; at five in the morning it was down at

79°; at nine the same morning, it had risen to 83°, and at one o'clock to 84°; and at nine in the evening it was down to 78°.

December 30th, air at 35°, bees at 73°.

Although bees support a heat nearly equal to that of a quadruped, yet their external covering is not different from that of insects which do not; there is no difference between their coat and a common fly's or wasp's, nor are they fatter, all which makes them bad retainers of heat; therefore they are chilly; and in a cold too severe for them to be comfortable in, they make up for their want of size singly, and get into clusters. A single bee has so little power of keeping itself warm, that it presently becomes numbed, and almost motionless; a common night in summer will produce this effect: a cold capable of producing such effects kills them soon, by which means vast numbers die; therefore a common bee is obliged to feed and live in society, to keep itself warm in cold weather. We know that the consumption of heat may be greater than the power of forming it; when that is the case, we become sensible of it, and then take on such actions as are either instinctive, such as arise naturally out of the impression, or as reason, custom, or habit direct. Many animals, upon the impression of cold, coil themselves up in their own fur, bringing all their extremities into the centre, or hollow of the belly; birds bring their feet under the belly, and thrust their bill between their wing and body; many, if not all, go to the warmest places, either from instinctive principle, or habit: but the bees have no other mode but forming clusters, and the larger the better. As they are easily affected by cold, their instinctive principle respecting cold is very strong, as likewise with regard to wet.

I have seen a swarm hanging out at the door of a hive, ready to take flight, and then return; a chill has come on, of which I was not sensible, and in a few minutes the whole has gone back into the hive; and by the cold increasing, I have at length perceived the cause of their return. If rain is coming on, we observe them returning home in great quantities, and hardly any abroad. The eggs of bees require this heat as much as themselves, nor will the maggot live in a cold of 60° or 70°, nor even their chrysalis. This warmth keeps the wax so soft, as to allow them to model it with ease. In glass hives, or those that have windows of glass in them, we often find a dew on the inside of the glass, especially when the glass is colder than the air within: whether this is perspiration from the bees, both from their external surface and lungs, or evaporation from the honey, I cannot say.

Bees are very cleanly animals respecting themselves, although not so respecting the remains of their young. They, I believe, seldom or never evacuate their excrement in the hive. I have known them confined many days without discharging the contents of the rectum; and the moment they got abroad, they evacuated in the air, when flying: and they appear to be very nice in their bodies, for I have often detected them cleaning one another, more especially if by accident they are besmeared with honey.

This animal may be considered alone, or so far as concerns its own œconomy as an individual, which is common to the most solitary animals; but it can also be considered as a member of society, in which it is taking an active part, and in which it becomes an object of great curiosity.

To consider this society individually, it may be said to consist of a female breeder, female non-breeders, and males: but to consider it as a community, it may be said to consist only of female breeders and non-breeders, the males answering no other purpose than simply as a male, and are only temporary; and it is probable, the female breeder is to be considered in no other light than as a layer of eggs, and that she only influences the non-breeders by her presence, being only a bond of union, for without her they seem to have no tie; it is her presence that makes them an aggregate animal. May we not suppose that the offspring of the queen have an attachment to the mother, somewhat similar to the attachment of young birds to the female that brings them up? for although the times of their attachment are not equal, yet it is the dependence which each has on its mother, that constitutes the bond; for bees have none without her: however, the similarity is not exact, for young animals who have lost their nurse will herd together, and jointly make the best shifts they can, because in future they are to become single animals; but bees have an eternal instinctive dependence on the mother, probably from there not being distinct sexes. When the queen is lost, this attachment is broke; they give up industry, probably die; or, we may suppose, join some other hive. This is not the case with those of this tribe, whose queen singly forms a colony; for although the queen is destroyed, yet they go on with that work which is their lot; as the wasp, hornet, and humble bee. Most probably the whole œconomy of the bee, which we so much admire, belongs to the non-breeders, and depends on their instinctive powers being set to work by the presence of the breeders, that being their only enjoyment; therefore when we

talk of the wonderful œconomy of bees, it is chiefly the labourers at large we are to admire, although the queen gets the principal credit, for the extent of *their* instinctive properties.

This œconomy, in its appearances and operations, is somewhat similar to human society, but very different in its first causes and mode of conduct. The human species sets up its own standard; the bee has one set up by nature, and therefore fulfils all the necessary purposes. This standard of influence, which is the breeder, is called the queen, and I shall keep to the name, although I do not allow her voluntary influence or power.

The non-breeders are what compose the hive, or what may be called the community at large; and the males, are mere males: each of these parts of the community I shall hereafter consider separately.

To take up the common bee in any one period of the year, or, in other words, in any one month, and carry it round to the same, and observe what happens in that time, is probably including the whole œconomy of bees; for although they may live more than one year, which I believe is not known, from its not being easily ascertained, yet each year can only be a repetition of the last, as I conceive they are complete in the first; therefore the history of one year may be said to make a whole, and of course it is not material at what time in the circle we begin the history.

Perhaps the best time to begin the history of such insects, as only come to full growth the season they are bred, and live through the winter, and breed the summer following, is when they emerge from the torpid state, and begin to breed; but it might be thought that the common bee is an exception to this

rule, because they begin early in the spring to breed, generally before they can be observed; and as they breed to form a colony, which is to go off from the old stock, in order to set out anew, it might seem most natural to begin with this colony, and trace it through its various actions of life for one year, when it, as it were, regenerates itself, and comes round to the same point again, that the old stock was in when it threw off this colony.

Bees, like every other animal that is taken care of in the time of breeding, or incubation, and nursed to the age of taking care of itself, cannot be said to have a period in which we can begin its natural history; but in some other insects there is such a period, for they can be traced from an egg, becoming totally independent of the parent from the moment of being laid, as the silk-worm, &c. There are three periods at which the history of the bee may commence: first, in the spring, when the queen begins to lay her eggs; in the summer, at the commencement of a new colony; or in the autumn, when they are going into winter-quarters. I shall begin the particular history of the bee with the new colony, when nothing is formed; for it begins then every thing that can possibly happen afterwards.

When a hive sends off a colony, it is commonly in the month of June, but that will vary according to the season, for in a mild spring bees sometimes swarm in the middle of May, and very often at the latter end of it. Before they come off, they commonly hang about the mouth of the hole, or door of the hive, for some days, as if they had not sufficient room within for such hot weather, which I believe is very much the case; for if cold or wet weather come on, they stow themselves very well, and wait for fine weather. But swarming appears to be

rather an operation arising from necessity, for they would seem not naturally to swarm, because if they have an empty space to fill, they do not swarm; therefore by increasing the size of the hive, the swarming is prevented. This period is much longer in some than in others. For some evenings before they come off, is often heard a singular noise, a kind of ring, or sound of a small trumpet; by comparing it with the notes of the piano-forte, it seemed to be the same sound with the lower A of the treble.

The swarm commonly consists of three classes; a female, or females,* males, and those commonly called mules, which are supposed to be of no sex, and are the labourers; the whole about two quarts in bulk, making about six or seven thousand. It is a question that cannot easily be determined, whether this old stock sends off entirely young of the same season, and whether the whole of their young ones, or only part. As the males are entirely bred in the same season, part go off; but part must stay, and most probably it is so with the others. They commonly come off in the heat of the day, often immediately after a shower; who takes the lead I do not know, but should suppose it was the queen. When one goes off, they all immediately follow, and fly about seemingly in great confusion, although there is one principle actuating the whole. They soon appear to be directed to some fixed place; such as the branch of a tree or bush, the cavities of old trees, holes of houses leading into some hollow place; and whenever the stand is made, they all immediately repair to it, till they are all collected. But it would seem, in some cases, that they had not fixed upon any resting

* I have reason to believe that never more than one female comes off with a swarm.

place before they came off, or if they had, that they were either disturbed, if it was near, or that it was at a great distance; for, after hovering some time, as if undetermined, they fly away, mount up into the air, and go off with great velocity. When they have fixed upon their future habitation, they immediately begin to make their combs, for they have the materials within themselves. I have reason to believe that they fill their crops with honey when they come away; probably from the stock in the hive. I killed several of those that came away, and found their crops full, while those that remained in the hive had their crops not near so full: some of them came away with farina on their legs, which I conceive to be rather accidental. I may just observe here, that a hive commonly sends off two, sometimes three swarms in a summer; but that the second is commonly less than the first, and the third less than the second; and this last has seldom time to provide for the winter: they shall often threaten to swarm, but do not; whether the threatening is owing to too many bees, and their not swarming is owing to there being no queen, I do not know. It sometimes happens that the swarm shall go back again; but in such instances I have reason to think that they have lost their queen, for the hives to which their swarm have come back do not swarm the next warm day, but shall hang out for a fortnight, or more, and then swarm; and when they do, the swarm is commonly much larger than before, which makes me suspect that they waited for the queen that was to have gone off with the next swarm.

So far we have set the colony in motion. The materials of their dwelling, or comb, which is the wax, is the next consideration, with the mode of forming, preparing, or disposing of

it. In giving a totally new account of the wax, I shall first show it can hardly be what it has been supposed to be. First, I shall observe that the materials, as they are found composing the comb, are not to be found in the same state (as a composition) in any vegetable, where they have been supposed to be got. The substance brought in on their legs, which is the farina of the flowers of plants, is, in common, I believe, imagined to be the materials of which the wax is made, for it is called by most the wax: but it is the farina, for it is always of the same colour as the farina of the flower where they are gathering; and indeed we see them gathering it, and we also see them covered almost all over with it, like a dust; nevertheless, it has been supposed to be the wax, or that the wax was extracted from it. REAUMUR is of this opinion. I made several experiments to see if there was such a quantity of oil in it, as would account for the quantity of wax to be formed, and to learn if it was composed of oil. I held it near the candle; it burnt, but did not smell like wax, and had the same smell, when burning, as farina when it was burnt. I observed that this substance was of different colours on different bees, but always of the same colour on both legs of the same bee; whereas new made comb was all of one colour. I observed, that it was gathered with more avidity for old hives, where the comb is complete, than for those hives where it is only begun, which we could hardly conceive if it was the materials of wax: also we may observe, that at the very beginning of a hive, the bees seldom bring in any substance on their legs for two or three days, and after that the farina gatherers begin to increase; for now some cells are formed to hold it as a store, and some eggs are laid, which when hatched will require this substance as

food, and which will be ready when the weather is wet. I have also observed, that when the weather has either been so cold, or so wet, in June, as to hinder a young swarm from going abroad, they have yet in that time formed as much new comb, as they did in the same time when the weather was such as allowed them to go abroad. I have seen them bring it in about the latter end of March, and have observed, in glass hives, the bees with the farina on their legs, and have seen them disposing of it, as will be described hereafter.

The wax is formed by the bees themselves; it may be called an external secretion of oil, and I have found that it is formed between each scale of the under side of the belly. When I first observed this substance, in my examination of the working bee, I was at a loss to say what it was: I asked myself if it was new scales forming, and whether they cast the old, as the lobster, &c. does? but it was to be found only between the scales, on the lower side of the belly. On examining the bees through glass hives, while they were climbing up the glass, I could see that most of them had this substance, for it looked as if the lower, or posterior edge of the scale, was double, or that there were double scales; but I perceived it was loose, not attached. Finding that the substance brought in on their legs was farina, intended, as appeared from every circumstance, to be the food of the maggot, and not to make wax; and not having yet perceived any thing that could give me the least idea of wax; I conceived these scales might be it, at least I thought it necessary to investigate them. I therefore took several on the point of a needle, and held them to a candle, where they melted, and immediately formed themselves into a round globe; upon which I no longer doubted but this was the wax, which opinion

was confirmed to me by not finding those scales but in the building season. In the bottom of the hive we see a good many of the scales lying loose, some pretty perfect, others in pieces. I have endeavoured to catch them, either taking this matter out of themselves, from between the scales of the abdomen, or from one another, but never could satisfy myself in this respect: however, I once caught a bee examining between the scales of the belly of another, but I could not find that it took any thing from between. We very often see some of the bees wagging their belly, as if tickled, running round, and to and fro, for only a little way, followed by one or two other bees, as if examining them. I conceived they were probably shaking out the scales of wax, and that the others were ready upon the watch to catch them, but I could not absolutely determine what they did. It is with these scales that they form the cells called the comb, but perhaps not entirely, for, I believe, they mix farina with it; however, this only occasionally, when probably the secretion is not in great plenty. I have some reason to think, that where no other substance is introduced, the thickness of the scale is the same with that of the sides of the comb; if so, then a comb may be no more than a number of these united; but a great deal of the comb seems to be too thick for this, and, indeed, would appear to be a mixture, similar to the covering of the chrysalis. The wax naturally is white, but when melted from the comb at large, it is yellow. I apprehended this might arise from its being stained with honey, the excrement of the maggots, and with the bee-bread. I steeped some white comb in honey, boiled some with farina, as also with old comb, but I could not say that it was made yellower. Wax, by

bleaching, is brought back to its natural colour, which is also a proof that its colour is derived from some mixture. I have reason to believe that they take the old comb, when either broken down, or by any accident rendered useless, and employ it again; but this can only be with combs that have had no bees hatched in them, for the wax cannot be separated from the silk afterwards. REAUMUR supposed that they new worked up the old materials, because he found the covering of the chrysalis of a yellower colour than the other parts of the new comb; but this is always so, whether they have old yellow comb to work up, or not, as will be shewn.

The bees who gather the farina, also form the wax, for I found it between their scales.

The cells, or rather the congeries of cells, which compose the comb, may be said to form perpendicular plates, or partitions, which extend from top to bottom of the cavity in which they build them, and from side to side. They always begin at the top, or roof of the vault, in which they build, and work downwards; but if the upper part of this vault, to which their combs are fixed, is removed, and a dome is put over, they begin at the upper edge of the old comb, and work up into the new cavity at the top. They generally may be guided as to the direction of their new plates of comb, by forming ridges at top, to which they begin to attach their comb. In a long hive, if these ridges are longitudinal, their plates of comb will be longitudinal; if placed transverse, so will be the plates; and if oblique, the plates of comb will be oblique. Each plate consists of a double set of cells, whose bottoms form the partition between each set. The plates themselves are not very regularly arranged, not forming a regular plane where they might have

done so; but are often adapted to the situation, or shape of the cavity in which they are built. The bees do not endeavour to shape their cavity to their work, as the wasps do, nor are the cells of equal depths, also fitting them to their situation; but as the breeding cells must all be of a given depth, they reserve a sufficient number for breeding in, and they put the honey into the others, as also into the shallow ones. The attachment of the comb round the cavity is not continued, but interrupted, so as to form passages; there are also passages in the middle of the plates, especially if there be a cross stick to support the comb; these allow of bees to go across from plate to plate. The substance which they use for attaching their combs to surrounding parts is not the same as the common wax; it is softer and tougher, a good deal like the substance with which they cover in their chrysalis, or the humble bee surrounds her eggs. It is probably a mixture of wax with farina. The cells are placed nearly horizontally, but not exactly so; the mouth raised a little, which probably may be to retain the honey the better; however this rule is not strictly observed, for often they are horizontal, and towards the lower edge of a plane of comb they are often declining. The first combs that a hive forms are the smallest, and much neater than the last, or lowermost. Their sides, or partitions between cell and cell, are much thinner, and the hexagon is much more perfect. The wax is purer, being probably little else but wax, and it is more brittle. The lower combs are considerably larger, and contain much more wax, or perhaps, more properly, more materials; and the cells are at such distances as to allow them to be of a round figure: the wax is softer, and there is something mixed with it. I have observed that the cells are not all of equal size, some being a

degree larger than the others; and that the small are the first formed, and of course at the upper part, where the bees begin, and the larger are nearer the lower part of the comb, or last made: however, in hives of particular construction, where the bees may begin to work at one end, and can work both down, and towards the other end, we often find the larger cells both on the lower part of the combs, and also at the opposite end. These are formed for the males to be bred in; and in the hornets and wasps combs, there are larger cells for the queens to be bred in: these are also formed in the lower tier, and the last formed.

The first comb made in a hive, is all of one colour, viz. almost white; but it is not so white towards the end of the season, having then more of a yellow cast.

Of the Royal Cell.

There is a cell, which is called the Royal Cell, often three or four of them, sometimes more; I have seen eleven, and even thirteen in the same hive; commonly they are placed on the edge of one or more of the combs, but often on the side of a comb; however, not in the centre, along with the other cells, like a large one placed among the others, but often against the mouths of the cells, and projecting out beyond the common surface of the comb; but most of them are formed from the edge of the comb, which terminates in one of these cells. The royal cell is much wider than the others, but seldom so deep: its mouth is round, and appears to be the largest half of an oval in depth, and is declining downwards, instead of being horizontal, or lateral. The materials of which it is composed are

softer than common wax, rather like the last mentioned, or those of which the lower edge of the plate of comb is made, or with which the bees cover the chrysalis: they have very little wax in their composition, not one third, the rest I conceive to be farina.

This is supposed to be the cell in which the queen is bred, but I have reason to believe that this is only imagination: for, first, it is too large, and, moreover, seldom so deep as the large cells in which the males are bred; whereas, if proportioned to the length of the queen, it ought to be deeper, for length of body is her greatest difference. In the second place, its mouth is placed downward; and in the third place, it is never lined with the silken covering of the chrysalis, similar to the cells of the males and labourers; nor do we find excrement at the bottom of it. The number of these cells is very different, in different hives. I think I have seen hives without any, and I have seen them with eleven or twelve, sometimes more. I have examined them at all times through the summer, but never found any alteration in them.

The comb seems at first to be formed for propagation, and the reception of honey to be only a secondary use; for if the bees lose their queen, they make no combs; and the wasp, hornet, &c. make combs, although they collect no honey; and the humble bee collects honey, and deposits it in cells she never made.

I shall not consider the bee as an excellent mathematician, capable of making exact forms, and having reasoned upon the best shape of the cell for capacity, so that the greatest number might be put into the smallest space (for the hornet and the wasp are much more correct, although not seemingly

under the same necessity, as they collect nothing to occupy their cells); because, although the bee is pretty perfect in these respects, yet it is very incorrect in others, in the formation of the comb: nor shall I consider these animals as forming comb of certain shape and size, from mere mechanical necessity, as from working round themselves; for such a mould would not form cells of different sizes, much less could wasps be guided by the same principle, as their cells are of very different sizes, and the first by much too small for the queen wasp to have worked round herself: but I shall consider the whole as an instinctive principle, in which the animal has no power of variation, or choice, but such as arises from what may be called external necessity. The cell has in common six sides, but this is most correct in those first formed; and their bottom is commonly composed of those sides, or planes, two of the sides making one; and they generally fall in between the bottoms of three cells of the opposite side; but this is not regular, it is only to be found where there is no external interruption.

I have already observed, that the last formed cells in the season are not so well made: that their partitions are thicker, and more of a yellow colour: this arises, I imagine, from the wax being less pure, having more alloy in it; and therefore, not being so strong, more of it is required. The bees would appear to reserve many of their cells for honey, and those are mostly at the upper part. In old hives, of several years standing, I have found the upper part of the comb free from the consequences of having bred, such as the silk lining, and the excrement of the maggots at the bottom; while the lower part, for probably more than one half of the plane of cells, shewed strong marks of having contained many broods of young bees.

In such the lining of silk is thick at the sides, composed of many laminæ; and in many, the bottom is half filled up with excrement; and I observed at such parts, the comb was thickest at its mouth, which inclines me to think, that when a cell becomes shallow, by the bottom being in some degree filled up, the bees then add to its mouth. Such also they seem to reserve principally for the bee-bread; so that to lay up a greater store of honey is an object to them.

Of the Laying of Eggs.

As soon as a few combs are formed, the female bee begins laying of eggs. As far as I have been able to observe, the queen is the only bee that propagates, although it is asserted that the labourers do. Her first eggs in the season are those which produce labourers; then the males, and probably the queen; this is the progress in the wasp, hornet, humble bee, &c. However, it is asserted by RIEM, that when a hive is deprived of a queen, labourers lay eggs; also, that at this time, some honey and farina are brought in, as store for a wet day. The eggs are laid at the bottom of the cell, and we find them there before the cells are half completed, so that propagation begins early and goes on along with the formation of the other cells. The egg is attached at one end to the bottom of the cell, sometimes standing perpendicularly, often obliquely; it has a glutinous, or slimy covering, which makes it stick to any thing it touches. It would appear that there was a period or periods for laying eggs; for I have observed in a new swarm, that the great business of laying eggs did not last above a fortnight; although the hive was not half filled with comb, it began to slacken.

Probably that end of the egg which is first protruded, is that which sticks to the bottom of the cell: and probably the tail of the maggot is formed at that end: when they move the egg, how they make it stick again, I do not know. I have just observed, that they often move the egg out of a cell, to some other, we may suppose; why they do this, I cannot say; whether it is because we have been exposing this part, is not easily determined. In those new formed combs, as also in many not half finished, we find the substance called bee-bread, and some of it is covered over with wax; which will be considered further. By the time they have worked above half way down the hive, with the comb, they are beginning to form the larger cells, and by this time the first broods are hatched, which were small, or labourers; and now they begin to breed males, and probably a queen, for a new swarm; because the males are now bred to impregnate the young queen for the present summer, as also for the next year. This progress in breeding is the same with that of the wasp, hornet, and humble bee.* Although *this account* is commonly allowed, yet writers on this subject have supposed another mode of producing a queen, when the hive is in possession of maggots, and deprived of their queen.

What may be called the complete process of the egg, namely, from the time of laying to the birth of the bee, (that is, the time of hatching,) the life of the maggot, and the life of the chrysalis, is, I believe, shorter than in most insects. It is not easy to

* REAUMUR on Bees, says, that the drone eggs, when laid in small cells, produce drones: and WILHELMI says, that it is the labourers only that lay drone eggs. Mr. RIEM says, that queens are never reared in any but royal cells, although males sometimes in common cells; and workers in old queen cells, but never in those recently made.

fix the time when the eggs hatch: I have been led to imagine it was in five days. When they hatch, we find the young maggot lying coiled up in the bottom of the cell, in some degree surrounded with a transparent fluid. In many of the cells, where the eggs have just hatched, we find the skin standing in its place, either not yet removed, or not pressed down by the maggot. There is now an additional employment for the labourers, namely, the feeding and nursing the young maggots. We may suppose the queen has nothing to do with this, as there are at all times labourers enough in the hive for such purposes, especially too, as she never does bring the materials, as every other of the tribe is obliged to do at first; therefore she seems to be a queen by hereditary, or rather, by natural right, while the humble bee, wasp, hornet, &c. seem rather to work themselves into royalty, or mistresses of the community. The bees are readily detected feeding the young maggot; and indeed a young maggot might easily be brought up, by any person who would be attentive to feed it. They open their two lateral pincers to receive the food, and swallow it. As they grow, they cast their coats, or cuticles; but how often they throw their coats, while in the maggot state, I do not know. I observed that they often removed their eggs; I also find they very often shift the maggot into another cell, even when very large. The maggots grow larger and larger till they nearly fill the cell; and by this time they require no more food, and are ready to be inclosed for the chrysalis state: how this period is discovered I do not know, for in every other insect, as far as I am acquainted, it is an operation of the maggot, or caterpillar itself; but in the common bee, it is an operation of the perfect animal; probably it arises from the maggot refusing food.

The time between their being hatched and their being inclosed is, I believe, four days; at least, from repeated observations, it comes nearly to that time: when ready for the chrysalis state, the bees cover over the mouth of the cell, with a substance of a light brown colour, much in the same manner that they cover the honey, excepting that, in the present instance, the covering is convex externally, and appears not to be entirely wax, but a mixture of wax and farina. The maggot is now perfectly inclosed, and it begins to line the cell and covering of the mouth above mentioned, with a silk it spins out similar to the silk-worm, and which makes a kind of pod for the chrysalis. BONNET observed, that, in one instance, the cell was too short for the chrysalis, and it broke its covering, and formed its pod higher, or more convex than common: this I can conceive possible; we often see it in the wasp. Having completed this lining, they cast off, or rather shove off, from the head backwards, the last maggot coat, which is deposited at the bottom of the cell, and then they become chrysalises.

Of the Food of the Maggot, or what is commonly called Bee-Bread.

One would naturally suppose, that the food of the maggot bee should be honey, both because it is the food of the old ones, and it is what they appear principally to collect for themselves; however, the circumstance of honey being food for the old ones is no argument, because very few young animals live on the same food with the old, and therefore it is probable the maggot bee does not live upon honey; and if we reason from analogy, we shall be led to suppose the bee-bread to be the food

of the maggot. It is the food of the maggot of the humble bee, who feeds upon honey, and even lays up a store of honey for a wet day, yet does not feed the young with it. It is the food of the maggot of a black bee, and also of several others of the solitary kind, who also feed upon honey; and wasps, &c. who do not bring in such materials, do not feed themselves upon honey. We cannot suppose, that the bee-bread is for the food of the old bees, when we see them collecting it in the months of June, July, &c. at which time they have honey in great plenty. This substance is as common to a hive as any part belonging to the œconomy of bees. Before they have formed five or six square inches of comb in a young hive, we shall find eggs, honey, and bee-bread; and at whatever time of the year we kill a hive, we shall find this substance; and if a hive is short of honey, and dies in the winter, we find no honey, but all the bee-bread, which was laid up in store for the maggots in the spring. They take great care of it, for it is often covered over with wax, as the honey, and I believe more especially in the winter; probably with a view to preserve it till wanted. In April I have found some of the cells full, others only half full. If we slit down a cell filled with this substance, we shall commonly find it composed of layers of different colours; some a deep orange, others a pale brown. In glass hives, we often find that the glass makes one side of the cell, and frequently in such we shall see at once the different strata above mentioned. This is the substance which they bring in on their legs, and consists of the farina of plants. It is not the farina of every plant that the bee collects, at least they are found gathering it from some with great industry, while we never find them on others: St. John's wort is a favourite

plant, but that comes late. The flower of the gourd, cucumber, &c. they seem to be fond of. What they do collect must be the very loose stuff, just ready to be blown off to impregnate the female part of the flower; and to shew that this is the case, we find bees impregnate flowers that have not the male part. It is in common of a yellow colour, but that of very different shades, often of an orange; and when we see bees collecting it on bushes that have a great many flowers, so as to furnish a complete load, it is then of the colour of the farina of that bush. It is curious to see them deposite this substance in the cell. On viewing the hives, we often see bees with this substance on their legs, moving along on the combs, as if looking out for the cell to deposite it in. They will often walk over a cell that has some deposited in it, but shall leave that, and try another, and so on till they fix; which made me conceive that each bee had its own cell. When they come to the intended cell, they put their two hind legs into it, with the two fore legs and the trunk out on the mouth of the neighbouring cell, and then the tail, or belly, is thrust down into the intended cell; they then bring the leg under the belly, and turning the point of the tail to the outside of the leg, where the farina is, they shove it off by the point of the tail. When it is thus shoved off both legs, the bee leaves it, and the two pieces of farina may be seen lying at the bottom of the cell: another bee comes almost immediately, and creeping into the cell, continues about five minutes, kneading and working it down into the bottom, or spreads it over what was deposited there before, leaving it a smooth surface.

It is of a consistency like paste; burns slightly, and gives a kind of unusual smell, probably from having been mixed with

animal juice in the act of kneading it down; for when brought in, it is rather a powder than a paste. That it is the food of the maggot is proved by examining the animal's stomach; for when we kill a maggot full grown, we find its stomach full of a similar substance, only softer, as if mixed with a fluid, but we never find honey in the stomach; therefore we are to suppose it is collected as food for the maggot, as much as honey is for the old bee. Mr. SCHIRACH imagines, that the semen of the male is the food of the maggot; but the food of the male and the queen maggot has been supposed to be different from that of the labourers. REAUMUR says, the food of the queen maggot is different in taste from that of the common ones. How he knew this, who was unacquainted with the food of the others, I cannot conceive.

Of the Excrement of the Maggot.

They have very little excrement, but what they do discharge is deposited at the bottom of the cell; and what at first will appear rather extraordinary, it is never cleared away by the bees, but allowed to dry along with the maggot coats; and both fresh eggs and honey are deposited in these cells, so circumstanced, every future year; so that in time the cells become nearly half full.

Of the Chrysalis State.

In this state they are forming themselves for a new life: they are either entirely new built, or wonderfully changed, for there is not the smallest vestige of the old form remaining;

yet it must be the same materials, for now nothing is taken in. How far this change is only the old parts new modelled, or gradually altering their form, is not easily determined. To bring about the change, many parts must be removed, out of which the new ones are probably formed. As bees are not different in this state from the common flying insects in general, I shall not pursue the subject of their changes further; although it makes a very material part in the natural history of insects.

When the chrysalis is formed into the complete bee, it then destroys the covering of its cell, and comes forth. The time it continues in this state is easier ascertained than either in that of the egg, or the maggot; for the bees cannot move the chrysalis, as they do the two others. In one instance it was thirteen days and twelve hours exactly; so that an egg in hatching being five days, the age of the maggot being four days, and the chrysalis continuing thirteen and a half, the whole makes twenty-two days and a half: but how far this is accurate, I will not pretend to say. I found that the chrysalis of a male was fourteen days, but this was probably accidental. When they first come out, they are of a greyish colour, but soon turn brown.

When the swarm of which I have hitherto been giving the history has come off early, and is a large one, more especially if it was put into too small a hive, it often breeds too many for the hive to keep through the winter; and in such case a new swarm is thrown off, which, however, is commonly not a large one, and generally has too little time to complete its comb, and store it with honey sufficient to preserve them through the winter. This is similar to the second or third swarm of the old hives.

Of the Seasons, when the different Operations of Bees take place.

I have already observed, that the new colony immediately sets about the increase of their numbers, and every thing relating to it. They had their apartments to build, both for the purpose of breeding, and as a storehouse for provisions for the winter. When the season for laying eggs is over, then is the season for collecting honey; therefore, when the last chrysalis for the season comes forth, its cell is immediately filled with honey, and as soon as a cell is full, it is covered over with pure wax, and is to be considered as store for the winter. This covering answers two very essential purposes: one is to keep it from spilling, or daubing the bees: the other to prevent its evaporation, by which means it is kept fluid in such a warmth. They are also employed in laying up a store of bee-bread for the young maggots in the spring, for they begin to bring forth much earlier than probably any other insect, because they retain a summer heat, and store up food for the young.

In the month of August we may suppose the queen, or queens, are impregnated by the males; and as the males do not provide for themselves, they become burdensome to the workers, and are therefore teased to death much sooner than they otherwise would die; and when the bees set about this business, of providing their winter store, every operation is over, except the collecting of honey and bee-bread. At this time it would seem as if the males were conscious of their danger, for they do not rest on the mouth of the hive in either

going out or coming in, but hurry either in, or out: however they are commonly attacked by one, two, or three at a time: they seem to make no resistance, only getting away as fast as possible. The labourers do not sting them, only pinch them, and pull them about as if to wear them out; but I suspect it may be called as much a natural, as a violent death.

The whole of the males are now destroyed, and indeed it would have been useless to have saved any to impregnate the queen in the spring. That there may be many more than may be wanted, I can easily believe, for this we see throughout nature; but she always times her operations well, although there may be supernumeraries.

When the young are wholly come forth, and either the cells entirely filled, or no more honey to be collected, then is the time, or season, for remaining in their hives for the winter.

Although I have now completed a hive, and no operations are going on in the winter months, yet the history of this hive is imperfect till it sends forth a new swarm.

As the common bee is very susceptible of cold, we find as soon as the cold weather sets in, they become very quiet, or still, and remain so throughout the winter, living on the produce of the summer and autumn; and indeed a cold day in the summer is sufficient to keep them at home, more so than a shower in a warm day: and if the hive is thin, and much exposed, they will hardly move in it, but get as close together as the comb will let them, into a cluster. In this manner they appear to live through the winter: however, in a fine day, they become very lively and active, going abroad, and appearing to enjoy it, at which time they get rid of their excrement; for I fancy they seldom throw out their excrement when in the

hive. To prove this, I confined some bees in a small hive, and fed them with honey for some days; and the moment I let them out, they flew, and threw out their excrement in large quantities; and therefore, in the winter, I presume, they retain the contents of their bowels for a considerable time: indeed, when we consider their confinement in the winter, and that they have no place to deposite their excrement, we can hardly account for the whole of this operation in them. Their excrement is of a yellow colour, and according to their confinement it is found higher and higher up in the intestine, almost as high as the crop.

Their life at this season of the year is more uniform, and may be termed simple existence, till the warm weather arrives again. As they now subsist on their summer's industry, they would seem to feed in proportion to the coldness of the season; for from experiment, I found the hive grow lighter in a cold week, than it did in a warmer, which led to further experiments. I first made an experiment upon a bee hive, to ascertain the quantity of honey lost through the winter. The hive was put into the scale November the 3d, 1776.

		oz.	drams.
November 10th	it had lost	2	7
17th	——	4	$2\frac{1}{2}$
24th	——	3	$7\frac{1}{2}$
December 1st	——	8	2
8th	——	2	1
15th	——	5	2
22d	——	4	3
29th	——	5	4
1777. January 1st	——	2	5

		oz.	drams.
January 12th	it had lost	5	2
19th	——	3	4
26th	——	3	$1\frac{1}{2}$
February 2d	——	5	0
9th	——	7	0
	The whole	72	$1\frac{1}{2}$

Although an indolent state is very much the condition of bees through the winter, yet progress is making in the queen towards a summer's increase. The eggs in the oviducts are beginning to swell, and, I believe, in the month of March she is ready to lay them, for the young bees are to swarm in June; which constitutes the queen bee to be the earliest breeder of any insect we know. In consequence of this, the labourers become sooner employed than any other of this tribe of insects. This both queen and labourers are enabled to accomplish, from living in society through the winter; and it becomes necessary in them, as they have their colony to form early in the summer, which is to provide for itself for the winter following. All this requires the process to be carried forward earlier than by any other insect, for these are only to have young which are to take care of themselves through the summer, not being under the necessity of providing for the winter.

In the month of April, I found in the cells, young bees, in all stages, from the egg to the chrysalis state; some of which were changed in their colour, therefore, were nearly arrived at the fly state, and probably some might have flown.

As this season is too early for collecting the provision of the maggot abroad, the store of farina comes now into use; but as

soon as flowers begin to blow, the bees gather the fresh, although they have farina in store, giving the fresh the preference.

Of the Queen.

The queen bee, as she is termed, has excited more curiosity than all the others, although much more belongs to the labourers. From the number of these, and from their exposing themselves, they have their history much better made out: but as there is only one queen, and she scarcely ever seen, it being only the effects of her labour we can come at, an opportunity has been given to the ingenuity of conjecture, and more has been said than can well be proved. She is allowed to be bred in the common way, only that there is a peculiar cell for her in her first stage; and REAUMUR says, "her food is dif-"ferent when in the maggot state;" but as there is probably but one queen, that the whole might not depend on one life, it is asserted that the labourers have a power of forming a common maggot into a queen. If authors had given us this as an opinion only, we might have passed it over as improbable, but they have endeavoured to prove it by experiments, which require to be examined: and for that purpose, I shall give what they say on that head, with my remarks upon it.

Abstracts from Mr. SCHIRACH.

The following experiments were made to ascertain the origin of the queen bee:—"In twelve wooden boxes were placed "twelve pieces of comb, four inches square, each containing

" both eggs and maggots, so suspended that the bees could " come round every part of the comb: in each box was shut " up a handful of working bees. Knowing that when bees " are forming a queen, they should be confined,* the boxes " were kept shut for two days. When examined at the end " of that period (six boxes only were opened), in all of them " royal cells were begun, one, two, or three, in each; all of " these containing a maggot four days old. In four days, the " other six boxes were opened, and royal cells found in each, " containing maggots five days old, surrounded by a large " provision of jelly; and one of these maggots, examined in " the microscope, in every respect resembled a working bee.

" This experiment was repeated, and the maggots se- " lected to be made queens were three days old; and in " seventeen days there were found in the twelve boxes fif- " teen lively, handsome queens.† These experiments were " made in May, and the bees were allowed to work great " part of the summer: the bees were examined one by one, " but no drone could be discovered, and yet the queens were " impregnated, and laid their eggs.‡

" The above experiment was repeated with pieces of comb,

* How he came to know this, I cannot conceive, for nothing *a priori* could give such information.

† Now this account is not only improbable, but it does not tally with itself. First, it is not probable that a handful of bees should, or would, set about making two, three, or four queens, when we do not find that number in a large hive: and secondly, it seems inconsistent that only fifteen should be formed out of twelve parcels, when some of the former parcels had four young queens.

‡ Here is a wonder of another kind: queens laying eggs, which (we must suppose Mr. SCHIRACH meant we should believe) they hatched, without the influence of the male.

" containing eggs only, in six boxes, but no preparations " were made towards producing a queen.*

" The experiment of producing a queen bee from a maggot " was repeated every month of the year, even in November.†

" A maggot three days old was procured from a friend, in- " closed in an ordinary cell, and shut up with a piece of comb, " containing eggs and maggots. That three days old was " formed into a queen, and all the other maggots and eggs " were destroyed.‡

" In above a hundred experiments a queen has been formed " from maggots three days old." §

WILHELMI observes, that a queen cell, which is made while the bees are shut up, is formed by breaking down three common cells into one, when the maggot is placed in the centre, after which the sides are repaired.

A young queen lately hatched was put into a hive, which had been previously ascertained to contain no drones, and whose queen was removed; and yet the young queen laid eggs.‖ In repeating Mr. SCHIRACH'S experiment, he shut up four

* Why eggs, which we must conceive hatched, and produced maggots, did not form queens, one cannot imagine.

† In which month, as bees never swarm, there could be no occasion for mothers, or supernumerary queens, and still each experiment produced a handsome queen. This is as singular an observation as any. In this country, and in all similar ones, bees hardly breed after July, and by the beginning of September there is hardly a chrysalis to be seen; yet these bred till November, and even laid eggs.

‡ Why did the bees destroy them in this experiment, and not in others?

§ The working bees, from the above experiments, are considered as all females, although the ovaria are too small for examination.

It would appear that a maggot three days old was of the best age for this experiment, yet one should have conceived that a maggot two days old would soon be fit.

‖ There is no mystery in this; but did they hatch?

pieces of comb, with one maggot in each; after two days the maggots were all dead, and the bees had desisted from labour. *

A piece of comb, from which all the eggs and maggots had been removed, was shut up with some honey, and a certain number of workers; in a short time they became very busy, and upon the evening of the second day 300 eggs were found in the cells. † He repeated this experiment with the same result, and the bees were left to themselves: they placed queen maggots in the queen cells, newly constructed, and others in male cells: the rest were left undisturbed. He again took two pieces of comb, which contained neither eggs nor maggots, and shut them up with a certain number of workers, and carried the box into a stove: next evening, one of the pieces of comb contained several eggs, and the beginning of a royal cell, that was empty.

Besides the short observations contained in the notes, I beg leave to observe, that I have my doubts respecting the whole of these experiments, from several circumstances which occurred in mine. The three following facts appear much against their probability: first, a summer's evening in this country is commonly too cold for so small a parcel of bees to be lively, so as to set about new operations; they get so benumbed, that they hardly recover in the day, and I should suspect that where these experiments were made (and indeed some are said to have been tried in this country), it is also too cold: secondly,

* This is the most probable event in the whole experiments.

† This would show that labourers can be changed into queens at will, and that neither they nor their eggs require to be impregnated; if this was the case, there would be no occasion for all the push in making a queen or a male.

if the weather should happen to be so warm as to prevent this effect, then they are so restless, that they commonly destroy themselves, or wear themselves out; at least, after a few days confinement we find them mostly dead: and thirdly, the account given of the formation of a royal cell, without mentioning the above inconvenience, which is natural to the experiment, makes me suspect the whole to be fabricated. To obviate the first objection, which I found from experiment to prevent any success that otherwise might arise, I put my parcel of bees, with their comb, in which were eggs, as also maggots, and in some of the trials there were chrysalises,* into a warmer place, such as a glass frame, over tan, the surface of which was covered with mould, to prevent the rising of unwholesome air: but from knowing that the maggot was fed with bee-bread, or farina, I took care to introduce a cell or two with this substance, as also the flowers of plants that produce a great deal of it, likewise some honey for the old ones. In this state my bees were preserved from the cold, as also provided with necessaries; but after being confined several days, upon opening the door of the hive, what were alive came to the door, walked and flew about, but gradually left it, and on examining the combs, &c. I found the maggots dead, and nothing like any operation going on.

* I chose to have some chrysalises, for I supposed that if my bees died, or flew away, the chrysalises when they came out, which would happen in a few days, not knowing where to go, might stay and take care of the maggots that might be hatched from the eggs; but, to my surprise, I found that neither the eggs hatched, nor did the chrysalises come forth; all died: from which I began to suspect that the presence of the bees was necessary for both.

The queen, the mother of all, in whatever way produced, is a true female, and different from both the labourers and the male. She is not so large in the trunk as the male, and appears to be rather larger in every part than the labourers. The scales on the under surface of the belly of the labourers are not uniformly of the same colour, over the whole scale; that part being lighter which is overlapped by the terminating scale above, and the uncovered part being darker: this light part does not terminate in a straight line, but in two curves, making a peak; all which gives the belly a lighter colour in the labouring bees: more especially when it is pulled out or elongated.

The tongue of the female is considerably shorter than that of the labouring bee, more like that of the male: however, the tongues of the labourers are not in all of an equal length, but none have it so short as the queen.

The size of the belly of the female of such animals varies a little, according to the condition they are in: but the belly of the male and the labourer has but little occasion to change its size, as they are at all times nearly in the same condition with regard to fat, having always plenty of provision: but the true female varies very considerably; she is of a different size and shape in the summer to what she is in the winter; and in the winter she has what may be called her natural size and shape: she is, upon the whole, rather thicker than the labourer; and this thickness is also in the belly, which probably arises from the circumstance of the oviduct being in the winter pretty large, and the reservoir for semen full. The termination of the belly is rather more peaked than in the labourers, the last scale being rather narrower from side to side, and com-

ing more to a point at the anus. The scales at this season are more overlapped, which can only be known by drawing them out. In the spring and summer she is more easily distinguished: the belly is not only thicker, but considerably longer than formerly, which arises from the increase of the eggs. We distinguish a queen from the working bee, simply by size, and in some degree by colour; but this last is not so easily ascertained, because the difference in the colour is not so remarkable in the back, and the only view we can commonly get of her is on this part; but when a hive is killed, the best way is to collect all the bees, and spread them on white paper, or put them into water, in a broad, flat-bottomed, shallow, white dish, in which they swim; and by looking at them singly, she may be discovered. As the queen breeds the first year she is produced, and the oviducts never entirely subside, an old queen is probably thicker than a new bred one, unless indeed the oviducts, and the eggs, form in the chrysalis state, as in the silk-worm, which I should suppose they did. The queen is perhaps at the smallest size just as she has done breeding, for as she is to lay eggs by the month of March, she must begin early to fill again; but I believe her oviducts are never emptied, having at all times eggs in them, although but small. She has fat in her belly, similar to the other bees.

It is most probable that the queen which goes off with the swarm is a young one, for the males go off with the swarm to impregnate her, as she must be impregnated the same year, because she breeds the same year.

The queen has a sting similar to the working bee.

Of the Number of Queens in a Hive.

I believe a hive, or swarm, has but one queen, at least I have never found more than one in a swarm, or in an old hive in the winter; and probably this is what constitutes a hive; for when there are two queens, it is likely that a division may begin to take place. Supernumerary queens are mentioned by RIEM, who asserts he has seen them killed by the labourers, as well as the males.

November 18th, 1788, I killed a hive that had not swarmed the summer before, and which was to appearance ready to swarm every day; but when I supposed the season for swarming was over, and it had not swarmed, I began to suspect that the reason why it did not was owing to there being no young queen or queens; and I found only one. This is a kind of presumptive proof that I was right in my conjecture; unless it be supposed, that when they were determined not to swarm, they destroyed every queen except one. In a hive that died, I found no males, and only one queen. This circumstance, that so few queens are bred, must arise from the natural security the queen is in from the mode of their society; for, although there is but one queen in a wasp's, hornet's, and humble bee's nest or hive, yet these breed a great number of queens; the wasp and hornet some hundreds; but not living in society during the winter, they are subject to great destruction, so that probably not one in a hundred lives to breed in the summer. I have said that the queen leaves off laying in the month of July; and now she is to be impregnated by the males before they die. Mr. RIEM asserts, he has seen the copulation

between the male and the female, but does not say at what season. I should doubt this; but Mr. SCHIRACH supposes the queen impregnated without copulation. I know not whether he means by this that she is not impregnated at all, and supposes, like Mr. DEBRAW, that the eggs are impregnated after they are laid, by a set of small drones, who pass over the cells, and thrust their tails down into the cell, so as to besmear the egg.* Mr. BONNET does not consider it necessary that the drones should be small for this purpose, for he saw a large drone passing over the cells of a piece of comb, stopping at every one which contained an egg, but at no other, and giving a knock with his tail on the mouth of the cell three times; this he supposed was the mode of impregnating the eggs. The number three has always been a famous number; but it will not do where there are no males, which is the case of a hive in the spring, the time when the queen is most employed in laying eggs; which made him suppose the use of the males was to feed the maggots with their semen. It is probable that the copulation is like that of most other insects. The copulation of the humble bee I have seen: it is similar to the common fly. The sting is extended at the time, and turned up on the back, between the two animals: they are some time in this act. In the hornet it is the same. The circumstances relative to the impregnating the queen not being known, great room has been given for conjecture, which, if authors had presented as conjectures only, it would have shewn their candour; but they have given, what in them were probably conceits, as facts.

* Mr. DEBRAW, knowing the drones died in the latter end of summer, or the autumn, was obliged to suppose a small set of males, that lived through the winter, for that purpose.

Of the Male Bee.

The male bee is considerably larger than the labourers: he is even larger than the queen, although not so long when she is in her full state with eggs: he is considerably thicker than either, but not longer in the same proportion: he does not terminate at the anus in so sharp a point; and the opening between the two last scales of the back and belly is larger, and more under the belly, than in the female. His proboscis is much shorter than that of the labouring bee, which makes me suspect he does not collect his own honey, but takes that which is brought home by the others; especially as we never find the males abroad on flowers, &c. only flying about the hives in hot weather, as if taking an airing; and when we find that the male of the humble bee, which collects its own food, has as long a proboscis, or tongue, as the female, I think it is from all these facts reasonable to suppose, the male of the common bee feeds at home. He has no sting.

The males, I believe, are later in being bred than the labouring bee. As they are only produced to go off with a hive, they are not so early brought forth; for in the month of April I killed a hive, in which I found maggots and chrysalises, but did not find any males among the latter: the maggots are too young for such investigation; but about the 20th of May we observed males: they are all very much of the same size. In the month of August, probably about the latter end, we may suppose they impregnate the queen for the next year, and about the latter end of the same month, and beginning of September, they are dying, but

seem to be hastened to their end by the labourers. In 1791, as early as the 19th of June, I saw the labourers killing the males of a hive, or rather of a swarm, that had not yet swarmed, but was hanging out; this, however, was out of the common course. They appear to be sensible of their fate, for they hurry in and out of the hive as quick as possible, seemingly with a view to avoid the labourers; and we find them attacked by the labourers, who pinch them with their forceps, and when they are so hurt, and fatigued with attempts to make their escape, as not to be able to fly, they are thrown over on the ground, and left to die. That this is the fate of every male bee is easily ascertained, by examining every bee in the hive when killed for the honey, which is after this season; no male being then found in it. BONNET supposes them starved to death, as he never saw wounds on them. In the course of a winter I have killed several hives, some as late as April, and in such a way as to preserve every bee, and after examining every one entirely, I never perceived one male of any kind; although it has been asserted there are two sizes of males, and that the small are preserved through the winter to impregnate the queen.

Of the Labouring Bee.

This class, for we cannot call it either sex, or species, is the largest in number of the whole community: there are thousands of them to one queen, and probably some hundreds to each male, as we shall see by and by. It is to be supposed they are the only bees which construct the whole hive, and that the queen has no other business but to lay the eggs: they are the only bees that bring in materials; the only ones we observe

busy abroad; and, indeed, the idea of any other is ridiculous, when we consider the disproportion in numbers, as well as the employment of the others, while the working bee has nothing to take off its attention to the business of the family. They are smaller than either the queen or the males: not all of equal size, although the difference is not very great.

The queen and the working bees are so much alike, that the latter would seem to be females on a different scale: however, this difference is not so observable in the beginning of winter as in the spring, when the queen is full of eggs. They are all females in construction, having the female parts, which are extremely small, and would be easily overlooked by a person not very well acquainted with the parts in the queen: this has been observed by Mr. RIEM; indeed, one might suppose that they were only young queens, and that they became queens after a certain age; but this is not the case. They all have stings, which is another thing that makes them similar to the queen. From their being furnished with an instrument of defence and offence, they are endowed with such powers of mind as to use it, their minds being extremely irritable; so much so, that they make an attack when not meddled with, simply upon suspicion, and when they do attack, they always sting; and yet, from the circumstance of their not being able to disengage the sting, one should suppose they would be more cautious in striking with it. When they attack one another, they seldom use it, only their pincers: yet I saw two bees engaged, and one stung the other in the mouth, or thereabouts, and the sting was drawn from the body to which it belonged, and the one who was stung ran very quickly about with it; but I could not catch that bee, to observe how the sting was situated.

As they are the collectors of honey, much more than what is for their own use, either immediately, or in future, their tongue is proportionably fitted for that purpose: it is considerably longer than that of either the queen or the male, which fits them to take up the honey from the hollow parts of flowers, of considerable depth. The mechanism is very curious, as will be explained further on.

The number of labourers in a hive varies very considerably.

In one hive that I killed, there were - -	3338
In another - - - -	4472
In one that died, there were - -	2432

That I might guess at the number of bees from a given bulk, I counted what number an alehouse pint held, when wet, and found it contained - - 2160

Therefore, as some swarms will fill two quarts, such must consist of near - - - - 9000

Of the Parts concerned in the Nourishment of the Bee.

Animals who only swallow food for themselves, or whose alimentary organs are fitted wholely for their own nourishment, have them adapted to that use only; but in many, these organs are common for more purposes, as in the pigeon, and likewise in the bee. In this last, some of the parts are used as a temporary reservoir, holding both that which is for the immediate nourishment of the animal, and also that which is to be preserved for a future day, in the cells formerly described; this last portion is therefore thrown up again, or regurgitated. As it is the labourers alone in the common bee that are so employed, we might conceive this reservoir would belong only

to them; but both the queen and males, both in the common and humble bee, have it, as also, I believe, every one of the bee tribe.

As the bee is a remarkable instance of regurgitation, it is necessary the structure of the parts concerned in this operation, and which are also connected with digestion, should be well considered. Ruminating animals may be reckoned regurgitating animals, but in them it is for the purpose of digestion entirely in themselves. But many birds may be called regurgitating animals, and in them it is for the purpose of feeding their young. Crows fill their fauces, making a kind of craw, out of which they throw back the food when they feed their young: but the most remarkable is the dove tribe, who first fill their craw, and then throw it up into the beak of their young*. The bee has this power to a remarkable degree, not, however, for the purpose of feeding the young, but it is the mode of depositing their store, when brought home.

In none of the above-mentioned regurgitating animals are the reservoirs containing the food, the immediate organ of digestion; nor does the reservoir for the honey in the bee appear to be its stomach.

The tongue of the bee is the first of the alimentary organs to be considered: it is of a peculiar structure, and is probably the largest tongue of any animal we know, for its size. It may be said to consist of three parts respecting its length, having three articulations. One, its articulation with the head, which is in some measure similar to our larynx. Then comes the body of the tongue, which is composed of two parts; one, a kind of base, on which the other, or

* *Vide* Observations on certain parts of the animal œconomy, p. 191.

true tongue, is articulated. This first part is principally a horny substance, in which there is a groove, and it is articulated with the first, or larynx; on the end of this is fixed the true tongue, with its different parts. These two parts of the tongue are as it were inclosed laterally, by two horny scales, one on each side, which are concave on that side next to the tongue; one edge is thicker than the other, and they do not extend so far as the other parts. Each of these scales is composed of two parts, or scales, respecting its length, one articulated with the other: the first of those scales is articulated with the common base, or larynx, at the articulation of the first part of the tongue, and incloses laterally the second part of the tongue, coming as far forwards as the third articulation: on the end of this is articulated the second scale, which continues the hollow groove that incloses the tongue laterally; this terminates in a point. These scales have some hairs on their edge.

On the termination of the second part, is placed the true tongue, having two lateral portions or processes, on each side, one within the other: the external is the largest, and is somewhat similar to the before-mentioned scales. This is composed of four parts, or rather of one large part, on which three smaller are articulated, having motion on themselves. The first, on which the others stand, is articulated at the edges of the tongue, on the basis, or termination of the last described part of the tongue: this has hairs on its edge.

A little further forwards on the edges of the tongue are two small thin processes, so small as hardly to be seen with the naked eye. The middle part of all, of which these lateral parts are only appendages, is the true tongue. It is something

longer than any of the before-mentioned lateral portions; and is not horny, as the other parts are, but what may be called fleshy, being soft and pliable. It is composed of short sections, which probably are so many short muscles, as in fish; for they are capable of moving it in all directions. The tongue itself is extremely villous, having some very long villi at the point, which act, I conceive, somewhat like capillary tubes.

This whole apparatus can be folded up, into a very small compass, under the head and neck. The larynx falls back into the neck, which brings the extreme end of the first portion of the tongue within the upper lip, or behind the two teeth; then the whole of the second part, which consists of five parts, is bent down upon and under this first part, and the two last scales are also bent down over the whole; so that the true tongue is inclosed laterally by the two second horny scales, and over the whole lie the two first.

The œsophagus, in all of this tribe of insects, begins just at the root of the tongue, as in other animals, covered anteriorly by a horny scale, which terminates the head, and which may be called the upper lip, or the roof of the mouth. It passes down through the neck and thorax, and when got into the abdomen, it immediately dilates into a fine transparent bag, which is the immediate receiver of whatever is swallowed. From this the food (whatever it be) is either carried further on into the stomach, to be digested, or is regurgitated for other purposes. To ascertain this in some degree, in living bees, I caught them going out early in the morning, and found this bag quite empty: some time after I caught others returning home, and found the bag quite full of honey, and some of it had

got into the stomach. Now I suppose that which was in the craw, was for the purpose of regurgitation; and as probably they had fasted during the night, part had gone on further for digestion. Whatever time the contents of this reservoir may be retained, we never find them altered, so as to give the idea of digestion having taken place: it is pure honey. From this bag the contents can be moved either way; either downwards to the stomach, for the immediate use of the animal itself; or back again, to be thrown out as store for future aliment.

The stomach arises from the lower end, and a little on the right side, of this bag. It does not gradually contract into a stomach, nor is the outlet a passage directly out, but in the centre of a projection which enters some way into the reservoir, being rather an inverted pylorus, thickest at its most projecting part, with a very small opening in the centre, of a peculiar construction. This inward projecting part is easily seen through the coats of the reservoir, especially if full of honey.

The stomach begins immediately on the outside of the reservoir, and the same part which projects into the reservoir, is continued some way into the stomach, but appears to have no particular construction at this end; and therefore it is only fitted to prevent regurgitation into the reservoir, as such would spoil the honey. This construction of parts is well adapted for the purpose; for the end projecting into the reservoir, prevents any honey from getting into the stomach, because it acts there as a valve; therefore whatever is taken in, must be by an action of this vascular part. The stomach has a good deal the appearance of a gut, especially as it seems to

come out from a bag. It passes almost directly downwards in the middle of the abdomen. Its inner surface is very much increased, by having either circular valves, somewhat like the *valvulæ conniventes* in the human *jejunum*, or spiral folds, as in the intestine of the shark, &c.; these may be seen through the external coats. In this part the food undergoes the change. Where the stomach terminates, is not exactly to be ascertained; but it soon begins to throw itself into convolutions, and becomes smaller.

The intestine makes two or three twists upon itself, in which part it is enveloped in the ducts, constituting the liver, and probably the pancreas, and at last passes on straight to the termination of the abdomen. Here it is capable of becoming very large, to serve upon occasion as a reservoir, containing a large quantity of excrement: it then contracts a little, and opens under the posterior edge of the last scale of the back, above the sting in the female and labourers, and the penis in the male.

Of the Senses of Bees.

Bees certainly have the five senses. Sight none can doubt. Feeling they also have; and there is every reason for supposing they have likewise taste, smell, and hearing. Taste we cannot doubt: but of smell we may not have such proofs: yet, from observation, I think they give strong signs of smell. When bees are hungry, as a young swarm in wet weather, and are in a glass hive, so that they can be examined, if we put some honey into the bottom, it will immediately breed a commotion; they all seem to be upon the scent: even if

they are weak, and hardly able to crawl, they will throw out their probosces as far as possible to get to it, although the light is very faint. This last appears to arise more from smell than seeing. If some bees are let loose in a bee hive, and do not know from which house they came, they will take their stand upon the outside of some hive, or hives; especially when the evening is coming on: whether this arises from the smell of the hives, or sound, I can hardly judge.

Of the Voice of Bees.

Bees may be said to have a voice. They are certainly capable of forming several sounds. They give a sound when flying, which they can vary according to circumstances. One accustomed to bees, can immediately tell when a bee makes an attack, by the sound. These are probably made by the wings. They may be seen standing at the door of their hive, with the belly rather raised, and moving their wings, making a noise. But they produce a noise independent of their wings; for if a bee is smeared all over with honey, so as to make the wings stick together, it will be found to make a noise, which is shrill and peevish. To ascertain this further, I held a bee by the legs, with a pair of pincers; and observed it then made the peevish noise, although the wings were perfectly still: I then cut the wings off, and found it made the same noise. I examined it in water, but it then did not produce the noise, till it was very much teased, and then it made the same kind of noise; and I could observe the water, or rather the surface of contact of the water with the air at the mouth of an air-hole

at the root of the wing, vibrating. I have observed that they, or some of them, make a noise the evenings before they swarm, which is a kind of ring, or sound of a small trumpet: by comparing it with the notes of the piano forte, it seemed to be the same with the lower A of the treble.

Of the Female Parts.

I may here observe, that insects differ from most of the classes of animals above them, in having their eggs formed in the ducts along which they pass; not in a cluster on the back, as in some fish (for instance all of the ray kind, or what are called the amphibia), in the bird, and as is supposed in the quadruped; from thence the eggs are taken up, and by the ducts are carried along to their places of destination.

Of the Oviducts.

The female of the common bee, similar to all the females of the bee tribe, has six oviducts on each side, beginning by very small, and almost imperceptible threads, as high as the chest: they then form one cord coiled up, or pass very serpentine, and become larger and larger as they approach the anus, owing to the gradual increased size of the eggs in them, which are now more distinct, and give the duct a sort of interrupted appearance, toward the lower end. The six ducts, when full of eggs, make a kind of quadrangle; then all unite into one duct, which enters the duct common to it and the oviducts of the other side. The ducts common to the six oviducts on each side, are extremely tender; so much so, that it is dif-

ficult to save them. The duct common to those on both sides may be called the vagina, and it is continued to the anus, or termination of the belly.

Of the Male Parts.

The male parts of generation, in the common bee, are much larger than in the humble bee. This we suppose necessary, considering the vast number of eggs the common bee lays, more than the humble bee does.

The external parts of generation of the male bee, are rather more under the belly, than in the others of this tribe; not so much at the termination of the belly; and they are rather more exposed, the two last scales, especially the under one, not projecting so much: the two holders are not so projecting beyond their base, nor are they so hooked, or sharp, as in the humble bee; hardly deserving the name of holders. From the external parts, passes up into the abdomen a pretty large sheath, whose termination incloses the glans penis. It is a bulbous part, having a dark coloured horny part upon it, which has two processes near its opening externally, one on each side, of a yellow colour: it has another process, which is white, and seems to be a gland. It can be made to pass along this sheath, or prepuce, and appear externally: I have been able, with a pair of forceps, to invert the sheath, beginning externally at the mouth, and pulling out a little at a time, by shifting my hold, till the glans has appeared externally.

The internal parts are the testicles, with their appendages. The testicles are two small oblong bodies, lying near the

back, having a vast number of air-vessels passing into them, and ramifying upon them. They are of a pale yellowish colour. From their lower ends pass down ducts, which may be called *vasa deferentia*, and which enter two bags: these two bags, into which the *vasa deferentia* enter, are probably reservoirs for the semen. From the union of these two bags passes out a duct, which runs towards the termination of the abdomen, and ends in the penis. These three parts, namely, testicles with their ducts, the two bags, and the duct arising from them, which I have termed urethra, are all folded on each other, so as to appear as one body.

In the introduction to this account of bees I observed, that several things in their œconomy might escape us if we considered them alone, but might be made out in other insects: an instance of this occurs in the impregnation of the female bee. The death of the males in the month of August, so that not one is left, and yet the queen to breed in the month of March, must puzzle any one not acquainted with the mode of impregnation of the females of most insects. Insects, respecting the males, are of two kinds: one, where the male lives through the winter, as well as the female; and the other, where every male of that species dies before the winter comes on; among which may be considered, as a third, those where both male and female die the same year. Of the first, I shall only give the common fly as an instance; of the second, I shall just mention all of the bee tribe; and the third may be illustrated in the silk-worm. The mode of impregnation in the first, is its being continued uninterruptedly through the whole period of laying eggs; while in the second, the copulation is in store; and, in the third the female lays up, by

the copulation, a store of semen, although the male is alive: of this I shall now give an explanation in the silk-moth, which may be applied to the bee, and many other insects.

In dissecting the female parts in the silk-moth, I discovered a bag lying on what may be called the vagina, or common oviduct, whose mouth, or opening, was external, but it had a canal of communication between it and the common oviduct. In dissecting these parts before copulation, I found this bag empty, and when I dissected them after, I found it full. Suspecting this to contain the semen of the male, I immediately conceived the following experiment: I opened the female as soon as the male had united to her, and found the penis in the opening of this bag, and by opening the duct where the penis lay, I observed the semen lying on the end of the penis. In another, I observed the bag to fill in the time of copulation: and in a pair that died in the act, I found the penis in this passage.

When we consider the impregnation of the egg in the silk-worm, we may observe the following circumstances:

First, many of the ova are completely formed, and covered with a hard shell, before copulation: secondly, the animals are a vast while in the act of copulation: and thirdly, the bags at the anus are filled during the time of copulation. From the first observation it appears, that the egg can receive the male influence through the hard or horny part of the shell. To know how far the whole, or only a part of the eggs, were impregnated by each copulation, I made the following experiments.* I took a female just emerged out of her cell, and put

* All these experiments on the silk-moth were begun in the summer 1767, and repeated by Mr. BELL in the year 1770.

a male to her, and allowed them to be connected their full time. They were in copulation ten hours. I then put her into a box by herself, and when she laid her eggs, I numbered the different parcels as she laid them, viz. 1, 2, 3, 4, 5; these eggs I preserved, and in the summer following I perceived that the No. 5 was as prolific as the No. 1; so that this one copulation was capable of impregnating the whole brood: and therefore the male influence must go either along the oviduct its whole length, and impregnate the incomplete eggs, as well as the complete, which appears to me not likely; or those not yet formed were impregnated from the reservoir in the act of laying: for I conceived that these bags, by containing semen, had a power of impregnating the egg as it passed along to the anus, just as it traversed the mouth of the duct of communication.

Finding that eggs completely formed, could be impregnated by the semen, and also finding that the before-mentioned bag was a reservoir for the semen till wanted, I wished next to discover if they could be impregnated from the semen of this bag; but as this must be done without the act of copulation, I conceived it proper, first, to see whether the ova of insects might be impregnated without the natural act of copulation, by applying the male semen over the ova, just as they were laid. The following experiments were made on the silk-moth:

EXPERIMENT I.

I took a female moth, as soon as she escaped from her pod, and kept her carefully by herself, upon a clean card, till she began to lay; then I took males that were ready for copula-

tion, opened them, exposing their seminal ducts, and after cutting into these, collected their semen with a hair pencil: with this semen I covered the ova, as soon as they passed out of the vagina. The card with these eggs, having a written account of the experiment upon it, I kept in a box by itself. In the ensuing season, eight of the ova hatched at the same time with others naturally impregnated. Thus, then, I ascertained that the eggs could be impregnated by art, after they were laid.

The ova laid by females that had not been impregnated, did not stick where they were laid: so that the semen would appear not only to impregnate the ova, but also to be the means of attaching them.

To know whether that bag in the female silk-moth, which increased at the time of copulation, was filled with the semen of the male, I made the following experiment:

EXPERIMENT II.

I took a female moth, as soon as she had escaped from the pod, and kept her on a card till she began to lay. I then took females that were fully impregnated before they began to lay, and dissected out that bag which I supposed to be the receptacle for the male semen; and wetting a camel hair pencil with this matter, covered the ova as soon as they passed out of the vagina. These ova were laid carefully on the clean card, and kept till the ensuing season, when they all hatched at the same time with those naturally impregnated.

This proves that this bag is the receptacle for the semen, and gradually decreases as the eggs are laid.

Of the Sting of the Bee.

I have observed that it is only the queen and the labourers that have stings; and this provision of a sting is perhaps as curious a circumstance as any attending the bee, and probably is one of the characters of the bee tribe.

The apparatus itself is of a very curious construction, fitted for inflicting a wound, and at the same time conveying a poison into that wound. The apparatus consists of two piercers, conducted in a groove, or director, which appears to be itself the sting. This groove is somewhat thick at its base, but terminates in a point; it is articulated to the last scale of the upper side of the abdomen by thirteen thin scales, six on each side, and one behind the rectum. These scales inclose, as it were, the rectum or anus all round; they can hardly be said to be articulated to each other, only attached by thin membranes, which allow of a variety of motions; three of them, however, are attached more closely to a round and curved process, which comes from the basis of the groove in which the sting lies, as also to the curved arms of the sting, which spread out externally. The two stings may be said to begin by those two curved processes at their union with the scales, and converging towards the groove at its base, which they enter, then pass along it to its point. They are serrated on their outer edges, near to the point. These two stings can be thrust out beyond the groove, although not far, and they can be drawn within it; and, I believe, can be moved singly. All these parts are moved by muscles, which we may suppose are very strong in them, much stronger than in other animals;

and these muscles give motion in almost all directions, but more particularly outwards. It is wonderful how deep they will pierce solid bodies with the sting. I have examined the length they have pierced the palm of the hand, which is covered with a thick cuticle: it has often been about the $\frac{1}{12}$ of an inch. To perform this by mere force, two things are necessary, power of muscles, and strength of the sting; neither of which they seem to possess in sufficient degree. I own I do not understand this operation. I am apt to conceive there is something in it distinct from simple force applied to one end of a body; for if this was simply the case, the sting of the bee could not be made to pierce by any power applied to its base, as the least pressure bends it in any direction: it is possible the serrated edges may assist, by cutting their way in, like a saw.

The apparatus for the poison consists of two small ducts, which are the glands that secrete the poison: these two lie in the abdomen, among the air-cells, &c.: they both unite into one, which soon enters into, or forms, an oblong bag, like a bladder of urine; at the opposite end of which passes out a duct, which runs towards the angle where the two stings meet; and entering between the two stings, is continued between them in a groove, which forms a canal by the union of the two stings to this point. There is another duct on the right of that described above, which is not so circumscribed, and contains a thicker matter, which, as far as I have been able to judge, enters along with the other: but it is the first that contains the poison, which is a thin, clear fluid. To ascertain which was the poison, I dipped points of needles into both, and pricked the back of the hand; and those

punctures that had the fluid from the first-described bags in them grew sore and inflamed, while the others did not. From the stings having serrated edges, it is seldom the bees can disengage them; and they immediately upon stinging endeavour to make their escape, but are generally prevented, as it were caught in their own trap; and the force they use commonly drags out the whole of the apparatus for stinging, and also part of the bowels; so that the bee most frequently falls a sacrifice immediately upon having effected its purpose. Upon a superficial view, one conceives, that the first intention of the bee having a sting is evident; one sees it has property to defend, and that therefore it is fitted for defence; but why it should naturally fall a sacrifice in its own defence, does not so readily appear: besides, all bees have stings, although all bees have not property to defend, and therefore are not under the same necessity of being so provided. Probably its having a sting to use, was sufficient for nature to defend the bee, without using it liberally; and the loss of a bee or two, when they did sting, was of no consequence; for it is seldom that more die.

I have now carried the operations of a hive, or the œconomy of the bee, completely round the year; in which time they revolve to the first point we set out at, and the continuance is only a repetition of the same revolutions as I have now described: but those revolutions occasion a series of effects in the comb, which effects in time produce variations in the life of the hive. Besides, there are observations that have little to do with the œconomy of a year, but include the whole of the life of this insect, or at least its hive.

Of the Life of the Bee.

I have observed that the life of the male is only one summer, or rather a month or two; and this we know from there being none in the winter, otherwise their age could not be ascertained, as it is impossible to learn the age of either the queen or labourers. Some suppose that it is the young bees which swarm; and most probably it is so: but I think it is probable also, that a certain number of young ones may be retained to keep up the stock, as we must suppose that many of the old ones are, from accidents of various kinds, lost to the hive; and we could conceive, that a hive three or four years old might not have an original bee in it, although a bee might live twice that time. But there must be a period for a bee to live; and if I were to judge from analogy, I should say, that a bee's natural life is limited to a certain number of seasons; viz. one bee does not live one year, another two, another three, &c. I even conceive that no individual insect of any species lives one month longer than the others of the same species. I believe this is the case with all insects; but the age of either a labourer or a queen may never be discovered. One might suppose that the life of a bee, and the time a hive can possibly last, would be nearly equal: although this is not absolutely necessary, because they can produce a succession, which they probably do; for I am very ready to imagine, that after the first brood in the season, all the last winter bees die, and the hive is occupied with this first brood; and that they breed the first swarm, or

that the old breed the whole of this season's breeding, and then die, and those that continue through the winter are the young; and if so, then they follow the same course with their progenitors.

The comb of a hive may be said to be the furniture and storehouse of the bees, which by use wear out; and from the description I have given, it will appear that the comb in time will be rendered unfit for use. I observed, that they did not clean out the excrement of the maggot, and that the maggot, before it moved into the chrysalis state, lined the cell with a silk, similar to many other insects. It lines the whole cell, top, sides, and bottom; the two last are permanent; and at the bottom it covers with this lining its own excrement.* Why the bee maggot is formed to do this, is, probably, because honey afterwards is to be put into this cell; so that the honey is laid into this last silken bag. How often they may breed in the same cell I do not know, but I have known them three times in the same season; each time the excrement has been accumulating, and the cell has been lined three times with silk. From this account we must see that a cell, in time, will be so far filled up as to render it unfit for breeding. On separating the lining of silk, which is easiest done at the bottom, on account of the dried excrement between each lining, I have counted above twenty different linings in one cell, and found the cell about one quarter, or one third, filled up: when such a cell, or a piece of comb with such cells, is steeped in water, so as to soften the excrement between the linings, they are sepa-

* This neither the wasp nor hornet do, although they do not clean out the excrement of their maggots.

rated from each other at the bottom by the swelling of the excrement, so that they can be easily counted. A piece of comb so circumstanced, when boiled for the wax, will keep its form, and the small quantity of wax is squeezed out at different parts, as if squeezed out of a sponge, and runs together into the crevices: while a piece of comb, that never has been bred in, even of the same hive, melts almost wholly down. It is this wax that has the fine yellow, while the other of the same hives, although brown, yet shall be white when melted; so that I was led to imagine the wax took its tinge from the farina, excrement, &c. but upon boiling pure wax with such materials, it was not tinged with this transparent yellow, only became dirty. In some of those cells that had probably been bred in twenty times, or more, when soaked so as to make the excrement swell, I have seen the bottom of the last lining rise even with the mouth, or top of the cell, so that the cavity of the cell was now full: in others, I have seen it rise higher than the mouth, so that the last formed layers were almost inverted, and turned inside out. A piece of such comb, consisting of two rows of cells, is to be considered as a mould, and the lining of silk and the excrement as the cast; when this is boiled, so as either to extract all the wax or mould, or to destroy its original regular formation which constituted the comb, and nothing is left but the cells of silk, &c. they all easily separate from each other, being only so many casts, with the mould destroyed; and the bottoms, which were indented into each other, are very perfect.

From the above account we must see that the combs of a hive can only last a certain number of years; however, to make them last longer, the bees often add a little to the mouth

of the cell, which is seldom done with wax alone, but with a mixture; and they sometimes cover the silk lining of the last chrysalis; but all this makes such cells clumsy, in comparison to the original ones.

To CHARLES BLAGDEN, *M.D. Sec.R.S.*

DEAR SIR,

IN a Paper on the *Chronology* of the HINDOOS, which the Society did me the honour to print in the LXXXth Volume of their Transactions, I have said (p. 566 and 570) that the era of *Bikramajit* commences in the year 56 before Christ, and that the year 1846 corresponds with our year 1790 (from April): but it appears from an examination of the numbers of an original *Benares* almanack, that I have calculated the elapsed time of that era one year short (by comparison with the era of *Salabán*, which being employed in astronomical computations is sufficiently ascertained), and that the year 1847, not 1846, should correspond with 1790, and the commencement of the era with the year 57 before Christ, both from April.

I am very desirous that this error should be noticed as soon as possible, and request that the correction may have a place in the Volume now printing.

I am, dear Sir, &c.

WM. MARSDEN.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1791.

1791	Time. H.	Time. M.	Therm without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	38,5	48,5	30,03		SSW	1	Cloudy.
	2	0	43	51,5	29,91		SSW	1	Cloudy.
2	8	0	33	48,5	29,64	0,090	WSW	1	Cloudy.
	2	0	43	49	29,30		SSW	1	Cloudy.
3	8	0	31	46	29,30	0,070	W	1	Fine.
	2	0	36,5	51	29,37		W	1	Fine.
4	8	0	35	48,5	29,28	0,025	SE	1	Fine.
	2	0	38	50,5	28,91		SE	2	Rain.
5	8	0	34	48	29,17	0,015	SW	2	Cloudy.
	2	0	39	51	29,26		SW	2	Rain.
6	8	0	34	48,5	29,37	0,010	W	2	Fine.
	2	0	42	53	29,40		WSW	2	Fine.
7	8	0	48	50	29,05	0,251	WSW	2	Cloudy; much wind
	2	0	49	55	29,05		W	2	Fair. [last night.
8	8	0	41	50,5	29,11	0,023	WNW	2	Fair; much wind last
	2	0	44	54	29,24		WNW	2	Fair. [night.
9	8	0	41	50	28,78	0,070	W	2	Rain; much wind last
	2	0	43	52	28,96		WNW	2	Cloudy. [night.
10	8	0	39	50,5	29,71		SW	1	Cloudy.
	2	0	49	55	29,68		SSW	1	Cloudy.
11	8	0	48	54	29,68	0,205	W	1	Rain; much wind last
	2	0	48	56	29,69		W	1	Cloudy. [night.
12	8	0	40	53	29,94		SW	1	Fair.
	2	0	46	56	29,78		SSW	2	Cloudy.
13	8	0	43	53	29,26	0,280	W	2	Cloudy.
	2	0	45	56	29,38		WNW	2	Fair.
14	8	0	40,5	53	29,78	0,050	W	1	Cloudy.
	2	0	47	55	29,58		SW	2	Cloudy.
15	8	0	34,5	53	30,01	0,040	W	1	Fine.
	2	0	43,5	56	29,94		SW	2	Cloudy.
16	8	0	47	53,5	29,82	0,016	SW	2	Cloudy.
	2	0	53	58	29,77		WSW	2	Cloudy.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Jan. 17	8	o	47	54	29,40		W	2	Cloudy.
	2	o	48	57	29,45		W	2	Cloudy.
18	8	o	40	53,5	29,47		S	2	Cloudy.
	2	o	43	57	29,10		S	2	Rain.
19	8	o	42	52,5	28,68	0,193	SSW	2	Cloudy.
	2	o	43	56,5	28,68		SW	2	Cloudy.
20	8	o	41	52,5	28,18	0,197	WSW	2	Cloudy.
	2	o	43	56	28,46		W	2	Fine.
21	8	o	37	53	29,08	0,160	WNW	2	Fine.
	2	o	42	56	29,32		WNW	2	Fine.
22	8	o	35	52	29,86		W	1	Fair.
	2	o	43	54,5	29,76		SSW	2	Cloudy.
23	8	o	46	54,5	29,71	0,101	W	1	Cloudy.
	2	o	46,5	57	29,88		WNW	1	Cloudy.
24	8	o	34,5	52	30,50		W	1	Fine.
	2	o	42	55	30,48		W	1	Cloudy.
25	8	o	41	52,5	30,27		W	1	Cloudy.
	2	o	47	54	30,20		W	2	Cloudy.
26	8	o	43	53,5	30,15		W	1	Cloudy.
	2	o	45,5	54	30,11		W	1	Cloudy.
27	8	o	41,5	53	29,80		W	1	Rain.
	2	o	42	54	29,73		WNW	1	Rain.
28	8	o	34	51,5	29,98	0,150	NW	1	Fair.
	2	o	36,5	54,5	30,08		NW	2	Fine.
29	8	o	37	48,5	29,95		W	1	Cloudy.
	2	o	43,5	51	29,85		W	1	Cloudy.
30	8	o	41	51	29,77	0,011	W	1	Cloudy.
	2	o	47	53	29,85		NW	1	Fair.
31	8	o	47,5	52	29,83		WNW	2	Fair.
	2	o	52	55	29,89		WNW	2	Fair.

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for February, 1791.

1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Feb. 1	7	0	45	53	29,82		W	1	Cloudy.
	2	0	49	55	29,63		W	2	Fair.
2	7	0	37	52	29,94		WNW	2	Fair.
	2	0	39	54	30,11		WNW	2	Fine.
3	7	0	30	49	30,20		NNW	2	Fine.
	2	0	37	51	30,28		NW	2	Fine.
4	7	0	30,5	49	30,44		NNW	2	Fine.
	2	0	38	51,5	30,43		NNW	2	Fair.
5	7	0	37	49,5	30,48		W	1	Foggy.
	2	0	42	52	30,44		WNW	1	Cloudy.
6	7	0	34	49,5	30,40		W	1	Cloudy.
	2	0	40,5	51,5	30,40		W	1	Cloudy.
7	7	0	41	50,5	30,33		W	1	Foggy.
	2	0	48	53	30,33		W	1	Cloudy.
8	7	0	41,5	51,5	30,38		W	1	Foggy.
	2	0	48	54,5	30,37		W	1	Cloudy.
9	7	0	41,5	52	30,36		W	1	Cloudy.
	2	0	47	54	30,28		W	1	Cloudy.
10	7	0	44	52	30,08		W	2	Cloudy.
	2	0	47,5	54	30,03		SW	2	Cloudy.
11	7	0	47	54	29,76		SW	2	Cloudy.
	2	0	47,5	57	29,61		SW	2	Cloudy.
12	7	0	35	54	30,10		W	2	Fine.
	2	0	45	56	30,20		W	2	Fine.
13	7	0	42	53	29,94	0,265	W	2	Rain.
	2	0	48,5	55	29,86		WSW	2	Rain.
14	7	0	49,5	55	29,86	0,033	WSW	2	Cloudy.
	2	0	52,5	56,5	29,85		WSW	2	Cloudy.
15	7	0	46	55	29,76	0,032	WSW	1	Cloudy.
	2	0	48	56,5	29,78		WSW	2	Cloudy.
16	7	0	39,5	53,5	29,84		W	2	Fair.
	2	0	46,5	55,5	30,01		W	1	Fair.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Feb. 17	7	0	32,5	53	30,21		SW	1	Fair.
	2	0	44	53,5	29,98		SW	2	Cloudy.
18	7	0	36,5	52,5	29,27	0,035	WSW	1	Fine.
	2	0	42	53,5	29,16		WSW	1	Fair.
19	7	0	34,5	51,5	29,30		E	1	Rain.
	2	0	40	54,5	29,56		NNW	1	Cloudy.
20	7	0	35,5	51	29,78	0,057	NE	1	Fine.
	2	0	42	53,5	29,67		NE	1	Cloudy.
21	7	0	38,5	51	29,52	0,096	E	1	Cloudy.
	2	0	43	53,5	29,54		S	1	Cloudy.
22	7	0	35,5	50	29,70		SE	1	Fine.
	2	0	47	53	29,74		S by W	1	Fair.
23	7	0	45	50,5	29,52	0,026	S	2	Cloudy.
	2	0	50	54	29,37		SSW	2	Cloudy.
24	7	0	33,5	51	29,86	0,089	WNW	1	Fine.
	2	0	43	54,5	29,98		WNW	1	Fine.
25	7	0	38	51	29,96		E	1	Cloudy.
	2	0	41	53	29,85		E	1	Rain.
26	7	0	33	48,5	29,68	0,240	NNE	2	Snow.
	2	0	35	51,5	29,80		N by E	2	Rain.
27	7	0	34,5	48,5	29,97		N	1	Fair.
	2	0	41	52	29,97		N	2	Fair.
28	7	0	34	49	30,06		N	2	Fair.
	2	0	41	51,5	30,08		NW	2	Fair.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	7	0	32	48	30,13		NNW	1	Cloudy.
	2	0	39	51	30,12		N	1	Fair.
2	7	0	32,5	48,5	30,24		N	1	Cloudy.
	2	0	39	52	30,27		NE	1	Hazy.
3	7	0	31,5	48	30,51		WNW	1	Fair.
	2	0	41	53	30,51		W	1	Fine.
4	7	0	42	50	30,46		W	1	Cloudy.
	2	0	52	56	30,47		W	1	Fine.
5	7	0	45	52	30,45		W	1	Cloudy.
	2	0	48	55	30,40		WNW	1	Cloudy.
6	7	0	37,5	51,5	30,24		WNW	1	Hazy.
	2	0	48	55	30,20		WNW	1	Hazy.
7	7	0	38	51,5	30,27		WNW	1	Fair.
	2	0	47	54,5	30,37		NNW	2	Fair.
8	7	0	33,5	50,5	30,58		NNW	2	Fine.
	2	0	43	54	30,63		NNW	2	Fine.
9	7	0	32,5	50	30,67		W	1	Fine.
	2	0	45	54,5	30,62		W	1	Fine.
10	7	0	32,5	51,5	30,60		W	1	Hazy.
	2	0	50,5	54,5	30,54		WNW	1	Fair.
11	7	0	37	53	30,47		W	1	Hazy.
	2	0	48,5	54	30,44		WNW	1	Hazy.
12	7	0	37,5	53	30,28		WNW	1	Hazy.
	2	0	50	56,5	30,21		W	1	Fine.
13	7	0	39	53,5	30,18		W	1	Fine.
	2	0	53	58	30,16		W	1	Fine.
14	7	0	42	54	30,14		W	1	Fair.
	2	0	54	57,5	30,25		WNW	1	Cloudy.
15	7	0	46	54	30,36		W	2	Cloudy.
	2	0	55	59,5	30,37		WSW	2	Fine.
16	7	0	48	57	30,44		W	2	Fair.
	2	0	55	61,5	30,44		WSW	2	Fine.

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1791	Time.		Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.	°	°	Inches.	Inches.	Points.	Str.	
Mar. 17	7	0	38	57	30,41		W	1	Cloudy.
	2	0	49	60	30,40		W	1	Fine.
18	7	0	38	58	30,28		W	1	Fair.
	2	0	55	59,5	30,30		W	1	Cloudy.
19	7	0	38	56,5	30,18		W	1	Fair.
	2	0	51,5	61	30,02		WSW	2	Fair.
20	7	0	46	55,5	29,60		SW	2	Cloudy.
	2	0	49	59,5	29,44		SW	2	Rain.
21	7	0	41	55	28,90	0,458	WNW	2	Cloudy.
	2	0	48	58,5	29,26		NW	2	Fair.
22	7	0	40	55	29,83		WNW	2	Fair.
	2	0	51,5	58,5	29,99		WNW	2	Fair.
23	7	0	50	57	30,05		W	2	Cloudy.
	2	0	55	62	30,14		W	2	Fair.
24	7	0	38,5	57	30,53		W	1	Hazy.
	2	0	50	59,5	30,51		NW	1	Fine.
25	7	0	40	56,5	30,43		W	1	Fair.
	2	0	50	60	30,30		SW	2	Fine.
26	7	0	44	57	29,83		E	1	Cloudy.
	2	0	44	58,5	29,75		W	1	Rain.
27	7	0	39,5	56,5	29,98	0,258	NNW	1	Cloudy.
	2	0	46	60	30,05		N	1	Cloudy.
28	7	0	42	53	30,12		WNW	1	Cloudy.
	2	0	45,5	58,5	30,11		WNW	1	Cloudy.
29	7	0	47	56,5	30,04		W	1	Cloudy.
	2	0	49	59	30,02		NE	1	Cloudy.
30	7	0	43	56	30,01		E	1	Cloudy.
	2	0	49	58	30,01		E	1	Cloudy.
31	7	0	43	55	30,08		E	1	Cloudy.
	2	0	51,5	58	30,08		E	1	Cloudy.

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for April, 1791.

1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
April 1	7	0	44	55,5	29,99		SW	1	Cloudy.
	2	0	52	58,5	29,93		SSW	1	Fair.
2	7	0	43	56	29,78		E	1	Fair.
	2	0	53	60,5	29,72		ESE	2	Fine.
3	7	0	45	57	29,75		E	2	Fine.
	2	0	55	61	29,80		E	2	Fine.
4	7	0	41	57	29,88		ENE	1	Fine.
	2	0	51	60	29,84		ENE	2	Fine.
5	7	0	43	57	29,70		ENE	1	Cloudy.
	2	0	47	59,5	29,60		ENE	1	Cloudy.
6	7	0	42	57	29,38	0,102	NE	1	Rain.
	2	0	42,5	57,5	29,44		W	1	Rain.
7	7	0	45	57,5	29,74	0,478	W	1	Fair.
	2	0	54	59	29,83		W	2	Fair.
8	7	0	47	57,5	29,93		E	1	Cloudy.
	2	0	61	59,5	29,90		SE	1	Cloudy.
9	7	0	49,	58	29,90		W	1	Cloudy.
	2	0	60	60,5	29,86		W	1	Fair.
10	7	0	47	57,5	29,96		W	1	Fair.
	2	0	58	61,5	29,98		W	1	Fair.
11	7	0	49	58,5	29,98	0,037	E	1	Cloudy.
	2	0	59	60,5	29,94		E	1	Cloudy.
12	7	0	45	58	29,95		W	1	Fair.
	2	0	60	62	29,97		W	1	Fair.
13	7	0	49	58,5	29,98		E	1	Cloudy.
	2	0	56	62	29,93		E	1	Cloudy.
14	7	0	53	60	29,99	0,170	W	1	Fair.
	2	0	59	63	30,01		W	1	Fine.
15	7	0	49,5	59,5	30,08		W	1	Fine.
	2	0	61	63	30,08		W	1	Hazy.
16	7	0	51	59	30,11		E	1	Fair.
	2	0	60	64	30,10		E	1	Fine.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	7	0	53	61,5	30,00		E	1	Fine.
	2	0	64	64	29,93		ESE	1	Fine.
18	7	0	52	60,5	29,77		SW	2	Cloudy.
	2	0	61	64	29,73		WNW	1	Fair.
19	7	0	49,5	60,5	29,69		WSW	2	Fair.
	2	0	61	63	29,62		WSW	2	Cloudy.
20	7	0	51	59,5	29.50	0,013	SW	2	Cloudy.
	2	0	59	62	29,49		SW	2	Cloudy.
21	7	0	52,5	61,5	29,40		WSW	1	Fair.
	2	0	61	62,5	29,34		SW	1	Cloudy.
22	7	0	51	64	29,14	0,105	SW	1	Rain.
	2	0	58	63,5	29,08		WSW	1	Cloudy.
23	7	0	48	58	29,15	0,090	WSW	2	Fine.
	2	0	55	62	29,16		WSW	2	Fair.
24	7	0	49	59,5	29,26	0,130	WSW	2	Fine.
	2	0	55	61,5	29,36		WSW	2	Fair.
25	7	0	47	59,5	29,83		WNW	1	Fine.
	2	0	52	61	29,89		WNW	2	Rain.
26	7	0	46	58	30,02	0,153	NW	1	Hazy.
	2	0	58	62	29,98		E	2	Hazy.
27	7	0	49	59	29,85	0,142	E	1	Cloudy.
	2	0	55	60	29,83		NE	1	Cloudy.
28	7	0	50	59	29,80		NW	1	Cloudy.
	2	0	58	60,5	29,82		W	1	Cloudy.
29	7	0	48	60	29,85	0,040	N	1	Cloudy.
	2	0	52	59	29,92		N	1	Cloudy.
30	7	0	46	58	29,95		N	2	Cloudy.
	2	0	45	59	29,91		N	2	Cloudy.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 1	7	0	45	58,5	29,81	0,061	N	1	Cloudy.
	2	0	48	58	29,77		N	1	Cloudy.
2	7	0	47	58	29,86	0,357	N	1	Rain.
	2	0	49	58,5	29,91		NNE	2	Cloudy.
3	7	0	43,5	57	30,01	0,086	NNE	2	Cloudy.
	2	0	48	58	30,01		N	2	Cloudy.
4	7	0	43	56	30,00		N	1	Cloudy.
	2	0	45	58	30,00		N	2	Cloudy.
5	7	0	43	55	29,95		N	1	Cloudy.
	2	0	51	57,5	29,95		N	2	Fair.
6	7	0	42	55,5	30,00	0,115	NE	2	Fair.
	2	0	49	57	30,07		NNE	2	Fair.
7	7	0	39	55	30,30		N	1	Fine.
	2	0	50,5	57	30,33		N	1	Cloudy.
8	7	0	43	55	30,37		W	1	Fine.
	2	0	57	58,5	30,30		NW	1	Fair.
9	7	0	49	56	30,15		W	1	Cloudy.
	2	0	60	58	30,09		W	1	Cloudy.
10	7	0	51,5	57,5	30,02		W	1	Cloudy.
	2	0	55	58,5	30,06		N	1	Cloudy.
11	7	0	47	57	30,08	0,065	W	1	Hazy.
	2	0	57	58	29,96		S	1	Cloudy.
12	7	0	49,5	57	29,72	0,070	W	1	Cloudy.
	2	0	54	57,5	29,63		SSE	1	Cloudy.
13	7	0	49,5	56,5	29,59		SSW	1	Fine.
	2	0	59	58	29,64		SSW	1	Cloudy.
14	7	0	50,5	57	29,95		W	1	Fine.
	2	0	62	59	30,02		WSW	1	Fine.
15	7	0	52	57,5	30,16		W	1	Fine.
	2	0	65,5	59,5	30,11		WSW	1	Cloudy.
16	7	0	55	58	30,00		W	1	Cloudy.
	2	0	60	59	30,02		W	1	Fair.

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1791	Time.	Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	°	°	Inches.	Inches.	Points.	Str.	
May 17	7 0	51	58,5	29,87		W	2	Cloudy.
	2 0	61	60	29,80		WNW	2	Cloudy.
18	7 0	49	58	29,86		W	2	Cloudy.
	2 0	58	60	29,92		W	2	Fair.
19	7 0	53	58	29,55		W	2	Cloudy.
	2 0	57	59	29,53		W	2	Cloudy.
20	7 0	50	57,5	29,97		W	2	Fine.
	2 0	58	58,5	29,96		W	2	Cloudy.
21	7 0	50	57,5	30,03		W	2	Cloudy.
	2 0	61	59	29,96		W	2	Fine.
22	7 0	50	58,5	29,94		W	2	Fair.
	2 0	60	60	29,97		W	2	Fair.
23	7 0	50,5	58	29,85		S	2	Cloudy.
	2 0	59	58,5	29,69		SW	2	Cloudy.
24	7 0	49	55	29,92	0,040	WSW	1	Fine.
	2 0	58	58	29,97		WNW	2	Cloudy.
25	7 0	49	57	30,11		SW	1	Cloudy.
	2 0	56,5	58	30,14		N	1	Cloudy.
26	7 0	50	57,5	30,32		ENE	1	Fine.
	2 0	60	58,5	30,30		NE	2	Fine.
27	7 0	53	58	30,33		N	1	Fine.
	2 0	66	60	30,30		E	1	Cloudy.
28	7 0	53	58	30,34		ENE	1	Fine.
	2 0	67,5	61,5	30,29		E	1	Fine.
29	7 0	51	60	30,27		NE	1	Cloudy.
	2 0	63	61,5	30,19		NNE	1	Fine.
30	7 0	51	58	30,18		NE	1	Cloudy.
	2 0	62,5	61,5	30,16		NE	1	Fair.
31	7 0	52	60	30,16		E	1	Cloudy.
	2 0	61	61,5	30,10		E	1	Fine.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
June 1	7	0	56	61	30,07		E	1	Hazy.
	2	0	69,5	64,5	30,03		SSW	1	Fine.
2	7	0	60	62,5	30,08		SW	1	Fine.
	2	0	79	66	30,08		S by E	1	Fair.
3	7	0	60	64	30,10		E	1	Fine.
	2	0	75,5	67	30,11		E	1	Fine.
4	7	0	63	66	30,10		E	1	Fine.
	2	0	76	68,5	30,10		E	1	Fine.
5	7	0	65	67	30,09		E	1	Fine.
	2	0	79,5	69	30,05		E	1	Fine.
6	7	0	63	67,5	30,13		ENE	1	Fine.
	2	0	79,5	69,5	30,15		E	1	Fine.
7	7	0	63	67,5	30,22		ENE	1	Fine.
	2	0	80	70,5	30,22		NNE	1	Fine.
8	7	0	64	69	30,14		NE	1	Cloudy.
	2	0	66	69	30,12		NNE	2	Cloudy.
9	7	0	53,5	66,5	30,05		NNE	2	Fair.
	2	0	67	68,5	30,01		N	2	Fine.
10	7	0	56	65	29,90		WNW	1	Fine.
	2	0	69	67	29,85		NW	1	Cloudy.
11	7	0	49	66	29,68		E	2	Cloudy.
	2	0	58	64	29,72		NW	2	Cloudy.
12	7	0	47	62	29,79		WNW	1	Cloudy.
	2	0	52	62	29,75		N	2	Fair.
13	7	0	47	60	29,79		NW	1	Fair.
	2	0	56	61	29,78		N	2	Fair.
14	7	0	47	59	29,81	0,046	NNW	1	Fine.
	2	0	57,5	59,5	29,79		N	1	Cloudy.
15	7	0	49	58,5	29,83		NNE	2	Fine.
	2	0	56	58,5	29,82		NNE	1	Cloudy.
16	7	0	48	58	29,44	0,058	N	2	Rain.
	2	0	58	59,5	29,39		N E	2	Cloudy.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
June 17	7	0	53	58,5	29,67	0,075	E	2	Cloudy.
	2	0	59,5	59	29,75		E	2	Cloudy.
18	7	0	52	59	29,79		NE	1	Cloudy.
	2	0	58	59,5	29,74		NE	1	Cloudy.
19	7	0	53	59	29,74	0,020	NE	2	Fine.
	2	0	64	61	29,72		NE	2	Fine.
20	7	0	50	59	29,74		NE	2	Cloudy.
	2	0	58	59	29,79		NE	1	Cloudy.
21	7	0	51	58,5	29,85		WNW	2	Cloudy.
	2	0	57	58	29,84		SW	2	Cloudy.
22	7	0	47,5	58	29,92	0,133	W	1	Fair.
	2	0	59	58,5	29,92		W	2	Fair.
23	7	0	50	57	29,95		W	2	Fine.
	2	0	65	58,5	30,00		W	2	Cloudy.
24	7	0	56	59	30,18		W	2	Fine.
	2	0	70,5	60	30,19		W	2	Fair.
25	7	0	60	60	30,22		W	1	Fair.
	2	0	72	62	30,16		W	2	Fair.
26	7	0	62	62	30,08		W	2	Fair.
	2	0	76	64	30,00		SW	2	Fair.
27	7	0	61	64	29,91		W	1	Fair.
	2	0	72,5	65	29,91		W	1	Cloudy.
28	7	0	58	63,5	30,10		W	1	Fine.
	2	0	74,5	65	30,10		SW	1	Fine.
29	7	0	62	64,5	30,02		S	1	Fine.
	2	0	79	68	29,91		S	2	Fine.
30	7	0	62	67	29,70		S	2	Cloudy.
	2	0	69	68	29,67		SW	2	Cloudy.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
July 1	7	0	59	67	29,68	0,037	WSW	1	Cloudy.
	2	0	71	67,5	29,88		WSW	1	Fair.
2	7	0	54,5	66,5	29,95	0,270	S	1	Rain.
	2	0	69,5	66,5	29,88		WSW	2	Cloudy.
3	7	0	57	65,5	29,86	0,087	WSW	1	Cloudy.
	2	0	71	67	29,83		WSW	2	Cloudy.
4	7	0	59	66	29,54	0,157	WSW	2	Fair.
	2	0	62,5	66	29,55		W	2	Rain.
5	7	0	53,5	64	29,76	0,115	W	2	Fine.
	2	0	67	64,5	29,81		W	2	Fair.
6	7	0	53	62	29,90	0,081	W	2	Fair.
	2	0	66,5	63,5	29,98		W	2	Fair.
7	7	0	52	61	30,09		W	2	Fair.
	2	0	66,5	62,5	30,08		W	2	Cloudy.
8	7	0	57	62	30,05		W	1	Cloudy.
	2	0	70,5	63,5	30,01		W	1	Cloudy.
9	7	0	54,5	62	30,04		W	1	Cloudy.
	2	0	66,5	62,5	30,04		W	1	Cloudy.
10	7	0	57	62	29,90		W	2	Cloudy.
	2	0	62	62,5	29,83		WNW	2	Cloudy.
11	7	0	57	62	29,44	0,361	NE	1	Rain.
	2	0	62	62	29,53		NE	2	Cloudy.
12	7	0	54	61	29,80	0,035	NW	2	Cloudy.
	2	0	62	61,5	29,88		NW	2	Cloudy.
13	7	0	56	61	29,90		WNW	1	Cloudy.
	2	0	63	62	29,96		WNW	1	Cloudy.
14	7	0	53	61	30,08		WNW	1	Fine.
	2	0	68	62	30,11		WNW	1	Fair.
15	7	0	55	62	30,24		W	1	Fine.
	2	0	73,5	63,5	30,24		NE	1	Fine.
16	7	0	57	63	30,15		NE	1	Fine.
	2	0	70,5	65	30,06		E	1	Fine.

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1791	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
July 17	7	0	61	64	30,01		E	1	Cloudy.
	2	0	75,5	60	30,00		E	1	Cloudy.
18	7	0	62	66	29,90		E	1	Cloudy.
	2	0	78,5	68	29,80		ESE	1	Cloudy.
19	7	0	60	66	29,77	0,030	WSW	2	Cloudy.
	2	0	72	68	29,77		WSW	1	Fair.
20	7	0	59	66	29,88		WSW	1	Fair.
	2	0	65	66	29,89		WSW	1	Cloudy.
21	7	0	58	64,5	30,00		W	1	Cloudy.
	2	0	71,5	66,5	30,04		W	1	Fair.
22	7	0	55,5	64,5	30,11		WNW	1	Fair.
	2	0	72	66,5	30,08		W	1	Fine.
23	7	0	59	64	29,90		WSW	2	Cloudy.
	2	0	71,5	66	29,88		W	2	Fair.
24	7	0	58	65	29,83		W	1	Cloudy.
	2	0	59	64,5	29,80		WSW	1	Rain.
25	7	0	54	63,5	29,70	0,301	SW	2	Cloudy.
	2	0	69,5	64,5	29,70		SW	2	Fair.
26	7	0	58	63	29,70		SW	2	Cloudy.
	2	0	67	65	29,74		SSW	2	Cloudy.
27	7	0	57	63,5	29,58	0,675	S	2	Rain.
	2	0	65	64,5	29,66		SW	2	Cloudy.
28	7	0	56	63,5	29,96	0,045	SW	1	Fair.
	2	0	68	64	29,97		WSW	2	Cloudy.
29	7	0	60	63	29,86		SW	2	Fair.
	2	0	73	65	29,80		SW	2	Fair.
30	7	0	59	64	29,85		SW	2	Cloudy.
	2	0	65	65	29,93		SW	2	Cloudy.
31	7	0	59	64	30,01		SW	1	Cloudy.
	2	0	72	65	29,95		SW	1	Fine.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Aug. 1	7	0	62	65	29,93		SW	1	Cloudy.
	2	0	70	66	29,94		SW	1	Cloudy.
2	7	0	57	65	30,12	0,100	SW	1	Fair.
	2	0	68	66	30,16		WNW	1	Cloudy.
3	7	0	55	64	30,32		W	1	Fine.
	2	0	67	65	30,32		WNW	1	Fair.
4	7	0	59	64	30,30		W	1	Cloudy.
	2	0	72	65,5	30,28		W	1	Fair.
5	7	0	59	64,5	30,31		W	1	Fair.
	2	0	74	66,5	30,29		W	1	Cloudy.
6	7	0	66	66	30,16		N	1	Cloudy.
	2	0	78,5	67	30,11		N	1	Fair.
7	7	0	66	66,5	30,04		W	1	Cloudy.
	2	0	76,5	68	30,02		W	1	Fair.
8	7	0	59	67	30,08		N	1	Fair.
	2	0	73	69,5	30,08		N	1	Fair.
9	7	0	58	67	30,18		E	1	Fair.
	2	0	68	68,5	30,13		E	1	Fine.
10	7	0	60	67	29,98		E	1	Cloudy.
	2	0	70	68	29,94		E	1	Cloudy.
11	7	0	61	67	29,90		NE	1	Cloudy.
	2	0	76,5	69	29,88		N	1	Fine.
12	7	0	60	68	29,85		E	1	Fair.
	2	0	74	69	29,86		S	2	Fair.
13	7	0	63	68	30,02		W	1	Cloudy.
	2	0	73,5	69	30,09		W	1	Fair.
14	7	0	63	68,5	30,20		E	1	Fine.
	2	0	75	72	30,12		SE	1	Fine.
15	7	0	67	70	30,00	0,053	SW	1	Fine; - much thunder & lightn last night.
	2	0	74,5	72	29,95	0,348	ESE	1	Cloudy.
16	7	0	67	70,5	29,88		WSW	1	Cloudy.
	2	0	74,5	72	29,90		WSW	1	Cloudy.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	61	70	30,00	0,056	SSW	2	Fine.
	2	0	68	70	30,05		WNW	1	Cloudy.
18	7	0	58	68	30,32		NNW	1	Fine.
	2	0	69	69	30,32		NNW	1	Fine.
19	7	0	58	67,5	30,44		NNW	1	Fine.
	2	0	67,5	68,5	30,46		NNE	1	Fair.
20	7	0	56	66	30,52		E	1	Fine.
	2	0	69	68	30,46		E	1	Fine.
21	7	0	58	66,5	30,37		ENE	1	Cloudy.
	2	0	66	67,5	30,28		E	1	Fine.
22	7	0	59	67	30,14		E	1	Fair.
	2	0	74	68	30,08		SE	1	Cloudy.
23	7	0	57,5	67	30,00		WSW	1	Hazy.
	2	0	76,5	69	29,99		W	1	Fine.
24	7	0	64	68	29,94		W	1	Cloudy.
	2	0	67,5	69	29,94		SW	1	Cloudy.
25	7	0	60	66	29,94		W	2	Cloudy.
	2	0	71,5	68	29,91		W	2	Cloudy.
26	7	0	57	66	29,88		W	2	Fair.
	2	0	68	67	29,95		W	2	Cloudy.
27	7	0	56,5	65,5	29,81	0,160	SSW	2	Rain.
	2	0	69,5	67	29,77		W.	1	Cloudy.
28	7	0	57	65	29,66	0,107	W	2	Cloudy.
	2	0	69,5	65,5	29,65		W	2	Fair.
29	7	0	57	64	29,74		WNW	1	Cloudy.
	2	0	65	65	29,85		WNW	1	Fair.
30	7	0	50	63	29,96		WNW	1	Fine.
	2	0	60	63	29,98		WNW	1	Cloudy.
31	7	0	51	62	29,99		E	1	Cloudy.
	2	0	54,5	62	29,81		SSW	1	Rain.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Sept. 1	7	0	52,5	62	29,68	0,276	NW	2	Cloudy.
	2	0	64	64	30,03		WNW	1	Fair.
2	7	0	54	62	30,12		SW	2	Cloudy.
	2	0	67	63,5	30,02		SW	2	Fair.
3	7	0	61	63	29,77	0,015	SW	2	Cloudy.
	2	0	66	63,5	29,68		SW	2	Cloudy.
4	7	0	56	64	29,52	0,191	W	2	Fine.
	2	0	67	65	29,54		W	2	Fair.
5	7	0	54	64	29,96		W	1	Fair.
	2	0	66	65	30,02		SW	1	Fair.
6	7	0	62	64	29,99		SW	2	Cloudy.
	2	0	66	65	29,99		SSW	2	Cloudy.
7	7	0	65	65	29,99		SW	2	Cloudy.
	2	0	70,5	66,5	30,05		WSW	2	Cloudy.
8	7	0	60	65	30,22		NE	1	Cloudy.
	2	0	66	65,5	30,22		E	1	Cloudy.
9	7	0	63	65,5	30,15		E	1	Cloudy.
	2	0	69	68	30,05		E	1	Fine.
10	7	0	61	67	30,05		E	1	Fine.
	2	0	76	71	30,06		E	1	Fine.
11	7	0	60	69	30,17		E	1	Fine.
	2	0	77	71	30,17		E	1	Fine.
12	7	0	58	69,5	30,17		E	1	Fine.
	2	0	73	71,5	30,17		E	1	Fine.
13	7	0	52	69	30,20		NE	1	Fine.
	2	0	73	70,5	30,20		E	1	Fine.
14	7	0	55	69	30,29		NE	1	Fine.
	2	0	75	70	30,29		N	1	Fine.
15	7	0	57,5	67	30,29		W	1	Fine.
	2	0	70,5	70	30,27		N	1	Fine.
16	7	0	52	67	30,33		W	1	Hazy.
	2	0	70	69	30,21		W	1	Fine.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Sept. 17	7	0	58	68	30,16		NNW	1	Cloudy.
	2	0	61,5	69	30,18		N	1	Fine.
18	7	0	53	66	29,79		W	1	Cloudy.
	2	0	60	65	29,74		WNW	1	Cloudy.
19	7	0	46	63	29,98		N	2	Fine.
	2	0	57	63,5	30,04		N	2	Fine.
20	7	0	43	61	30,10		W	1	Fine.
	2	0	60	61	30,08		WNW	1	Cloudy.
21	7	0	53	61	30,12		W	1	Cloudy.
	2	0	59	60,5	30,08		W	1	Cloudy.
22	7	0	51	60	30,10		N	1	Cloudy.
	2	0	59	62	30,08		N	1	Fine.
23	7	0	52,5	61	30,00		WNW	1	Cloudy.
	2	0	61	61	30,00		NW	1	Cloudy.
24	7	0	54	60	30,05		NNE	1	Cloudy.
	2	0	61	61	30,11		NNE	1	Cloudy.
25	7	0	49	60	30,27		NNE	1	Fine.
	2	0	62	61	30,28		NNE	1	Fair.
26	7	0	48	60	30,32		NNE	1	Fine.
	2	0	58	60	30,26		NE	1	Cloudy.
27	7	0	48	59	30,25		ENE	1	Cloudy.
	2	0	58	60	30,23		E	1	Fair.
28	7	0	50	58,5	30,25		NE	1	Fair.
	2	0	60,5	61,5	30,27		E	1	Fine.
29	7	0	46	58,5	30,31		NE	1	Fine.
	2	0	58,5	60,5	30,30		E	1	Fine.
30	7	0	48	59	30,25		NE	1	Fine.
	2	0	56	60	30,19		E	1	Fine.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Oct. 1	7	0	41	58	30,04		E	1	Cloudy.
	2	0	59	59,5	29,99		W	1	Fair.
2	7	0	50	58	30,00		W	1	Fair.
	2	0	62	59,5	30,00		WNW	1	Cloudy.
3	7	0	54	58,5	30,17		NNE	1	Hazy.
	2	0	61,5	60	30,06		SSW	1	Cloudy.
4	7	0	56	59,5	29,94	0,100	ESE	1	Rain.
	2	0	57,5	60	29,87		S by E	1	Cloudy.
5	7	0	51	59,5	29,81	0,220	ENE	1	Fair.
	2	0	62,5	61,5	29,80		E	1	Fine.
6	7	0	56	60	29,80	0,012	WSW	2	Cloudy.
	2	0	61,5	60,5	29,77		SW	2	Cloudy.
7	7	0	51,5	60	29,77	0,035	W	1	Cloudy.
	2	0	58,5	60,5	29,75		W	1	Cloudy.
8	7	0	48	59	29,76		WNW	1	Fair.
	2	0	57	61	29,78		NW	1	Fair.
9	7	0	47	59	29,53	0,105	SSE	1	Rain.
	2	0	58	59	29,23		SSE	2	Rain.
10	7	0	49	58,5	29,22		S	1	Fair.
	2	0	58,5	60,5	29,27		SSW	2	Fair.
11	7	0	47	58,5	29,30	0,120	W	1	Fine.
	2	0	55	59	29,39		W	2	Fine.
12	7	0	41	57	29,74		W	1	Fair.
	2	0	54	58,5	29,79		N	1	Fine.
13	7	0	43	57	29,16		W	1	Fine.
	2	0	53	58,5	29,60		E	1	Fair.
14	7	0	49	57	29,24		E	1	Hazy.
	2	0	54	58	29,18		SSW	2	Cloudy.
15	7	0	44	56	29,38		E	1	Fine.
	2	0	53,5	58	29,34		E	1	Fine.
16	7	0	40	55,5	29,44		W	1	Hazy.
	2	0	53	56,5	29,50		W	1	Fair.

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1791	Time.	Therm. without.	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	°	°	Inches.	Inches.	Points.	Str.	
Oct. 17	7 0	39,5	54	29,55		W	1	Cloudy.
	2 0	52	57	29,52		E	1	Cloudy.
18	7 0	53	57	29,27	0,161	SSW	2	Cloudy.
	2 0	56,5	58	29,41		W	2	Cloudy.
19	7 0	49	57	29,44	0,143	WSW	1	Cloudy.
	2 0	56	58	29,40		SW	1	Cloudy.
20	7 0	52	57,5	29,00	0,441	WSW	2	Cloudy.
	2 0	60	60	29,00		WSW	2	Fair.
21	7 0	54	58	28,89	0,081	SW	2	Cloudy.
	2 0	57	60,5	28,94		SW	2	Cloudy.
22	7 0	50	57,5	29,17	0,073	WNW	1	Cloudy.
	2 0	54	60	29,29		WNW	1	Cloudy.
23	7 0	39,5	57	29,66		WNW	1	Fine.
	2 0	46	60	29,76		WNW	1	Fine.
24	7 0	34	54	30,02		WNW	1	Fine.
	2 0	43	58,5	30,06		WNW	1	Fine.
25	7 0	37	54	29,92		E	1	Fine.
	2 0	49	57,5	29,77		SSE	2	Cloudy.
26	7 0	41	54	29,59	0,494	E	1	Cloudy.
	2 0	46	58	29,65		E	1	Cloudy.
27	7 0	41	54	30,02	0,042	NNE	1	Cloudy.
	2 0	48,5	58	30,15		NNE	1	Cloudy.
28	7 0	39	53,5	30,42		NNE	1	Fair.
	2 0	48,5	58	30,46		NE	1	Fair.
29	7 0	36	54	30,46		ENE	1	Fine.
	2 0	49,5	57,5	30,44		E	1	Cloudy.
30	7 0	42	54	30,30		NNE	1	Cloudy.
	2 0	47,5	55	30,22		E	2	Cloudy.
31	7 0	40	52,5	30,05		E	1	Cloudy.
	2 0	44,5	55,5	30,00		E	2	Cloudy.

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1791	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Nov. 1	7 0	41	53	29,86		NNE	2	Cloudy.
	2 0	39	55	29,86		NE	2	Cloudy.
2	7 0	36	52	29,77		NE	1	Cloudy.
	2 0	42	53	29,67		NE	1	Cloudy.
3	7 0	36	51	29,63	0,324	NNE	1	Cloudy.
	2 0	40,5	53	29,66		ENE	1	Cloudy.
4	7 0	37	51,5	29,65	0,030	NW	1	Cloudy.
	2 0	46	53	29,64		NNE	1	Cloudy.
5	7 0	37	51,5	29,68		N	1	Cloudy.
	2 0	46	53	29,79		N	1	Cloudy.
6	7 0	32,5	50	30,13		N	1	Cloudy.
	2 0	38	51	30,18		NW	1	Fine.
7	7 0	25	48	30,22				Foggy.
	2 0	36	49	30,22		WNW	1	Foggy.
8	7 0	32,5	48	30,18		SSE	1	Fair.
	2 0	41	51,5	30,13		SW	1	Fine.
9	7 0	39	49,5	30,08		W	1	Cloudy.
	2 0	49	52,5	30,05		SW	1	Cloudy.
10	7 0	38	50,5	30,15	0,205			Foggy.
	2 0	50	53,5	30,11		S	1	Rain.
11	7 0	48,5	53	30,03	0,167	S	2	Cloudy.
	2 0	49	57	29,95		S	2	Fair.
12	7 0	42	52	29,58		SSE	2	Fine.
	2 0	48	56	29,51		S	2	Cloudy.
13	7 0	42,5	52	29,57	0,050	SSE	2	Cloudy.
	2 0	48,5	56	29,47		SSE	2	Cloudy.
14	7 0	45	53	29,05		E	1	Cloudy.
	2 0	49	56	29,03		E	1	Cloudy.
15	7 0	37	53	29,39	0,101	W	1	Fine.
	2 0	47	56	29,29		W	1	Fair.
16	7 0	42	53	28,88	0,248	WSW	2	Cloudy.
	2 0	49	56	29,03		W	2	Cloudy.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Nov. 17	7	0	42	54	29,20		W	1	Cloudy.
	2	0	46	56	29,05		NE	1	Cloudy.
18	7	0	43	55,5	28,80	0,450	W	1	Rain.
	2	0	43	56	28,90		WSW	1	Cloudy.
19	7	0	46	55	28,76	0,241	E	1	Rain.
	2	0	50	58	28,89		ESE	2	Cloudy.
20	7	0	41	56	29,42	0,130	NW	1	Rain.
	2	0	48	57	29,47		NE	1	Cloudy.
21	7	0	43	55,5	29,49	0,150	NW	1	Cloudy.
	2	0	48	57	29,69		W	1	Cloudy.
22	7	0	41	55	29,94		SE	1	Fair.
	2	0	48	57,5	29,97		SSW	2	Fine.
23	7	0	47	56	30,01		S	2	Fair.
	2	0	50	58	30,01		SSE	1	Cloudy.
24	7	0	50	57	29,81	0,301	WSW	1	Cloudy.
	2	0	52,5	58	29,87		SW	1	Cloudy.
25	7	0	45	57	29,66		WSW	1	Fair.
	2	0	49	58,5	29,84		WSW	1	Fair.
26	7	0	45	57	30,13		W	1	Fine.
	2	0	48	59,5	30,26		W	2	Fine.
27	7	0	44	56	30,28		W	2	Cloudy.
	2	0	50,5	58	30,17		W	2	Cloudy.
28	7	0	52	58	29,70		W	1	Rain.
	2	0	52	58,5	29,53		WNW	2	Fine.
29	7	0	42	56	29,47	0,130	WNW	2	Fine.
	2	0	46	57	29,63		WNW	1	Fair.
30	7	0	35	54	29,78		SW	1	Cloudy.
	2	0	43	56	29,57				

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Dec. 1	8	0	38	53	29,58	0,102	WNW	1	Fine.
	2	0	44	56	29,62		W	2	Cloudy.
2	8	0	46	54	29,24		W	2	Cloudy.
	2	0	46	55,5	29,33		W	1	Cloudy.
3	8	0	37	54	29,55		WNW	1	Cloudy.
	2	0	40	55	29,51		ENE	2	Cloudy.
4	8	0	37	53,5	28,90	0,353	W	2	Cloudy; much wind
	2	0	39,5	53	29,27		NW	2	Cloudy. [last night.
5	8	0	31,5	52,5	29,75		WNW	1	Cloudy.
	2	0	34	52	29,74		SW	1	Foggy.
6	8	0	30	49,5	29,66				Foggy.
	2	0	40	50,5	29,66		SSW	1	Cloudy.
7	8	0	37	49,5	29,57		SW	1	Cloudy.
	2	0	44	51	29,43		SW	1	Cloudy.
8	8	0	34	49	29,31	0,067	WNW	1	Fine.
	2	0	38	52,5	29,38		WNW	1	Fine.
9	8	0	31,5	49	29,44		WNW	1	Fair.
	2	0	35	50	29,38		WNW	1	Cloudy.
10	8	0	31,5	48	29,31		WNW	1	Cloudy.
	2	0	35	50,5	29,35		WNW	1	Fair.
11	8	0	26	47	29,61		NW	2	Fine.
	2	0	31	48	29,68		WNW	1	Cloudy.
12	8	0	21	45	29,91		W	1	Fine.
	2	0	33	46,5	29,61		S	2	Cloudy.
13	8	0	40	46	29,11	0,045	W	1	Cloudy.
	2	0	42	49	29,10		W	1	Cloudy.
14	8	0	35	47	29,19		W	1	Cloudy.
	2	0	38	49	29,30		N	1	Cloudy.
15	8	0	33,5	47	29,65		N	1	Cloudy.
	2	0	38	48,5	29,78		NNE	1	Cloudy.
16	8	0	28	46	30,10		NNE	1	Fine.
	2	0	36	49	30,18		NNE	1	Fine.

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1791	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain. Inches.	Winds. Points.	Str.	Weather.
Dec. 17	8	0	29	45,5	30,38		NE	1	Fine.
	2	0	32	48,5	30,38		W	1	Fine.
18	8	0	30	46	30,23		W	1	Cloudy.
	2	0	35	47,5	30,16		WNW	1	Hazy.
19	8	0	35	47	30,00		NE	1	Cloudy.
	2	0	39	49	30,04		NE	1	Cloudy.
20	8	0	33	46	30,04		NNE	1	Cloudy.
	2	0	35	51	30,07		NNE	1	Fair.
21	8	0	33	47	30,13		NNE	1	Cloudy.
	2	0	36	51,5	30,10		NE	1	Fair.
22	8	0	30	46,5	29,96		NE	1	Cloudy.
	2	0	34	48	29,87		SSW	1	Fair.
23	8	0	43	47	29,20	0,170	SW	2	Cloudy.
	2	0	48	52	29,03		WNW	2	Cloudy.
24	8	0	33	49	29,33		WNW	2	Cloudy.
	2	0	37	49,5	29,49		WNW	2	Cloudy.
25	8	0	40	48,5	29,45	0,900	SSW	2	Cloudy.
	2	0	43	52,5	29,37		WSW	2	Cloudy.
26	8	0	34	49	29,31		WNW	1	Fine.
	2	0	40	53	29,53		WNW	1	Fine.
27	8	0	36	49	29,68		WSW	1	Cloudy.
	2	0	42	52,5	29,35		S	2	Cloudy.
28	8	0	42	50	29,29		W	2	Cloudy.
	2	0	43	54	29,58		NW	2	Fine.
29	8	0	36	50	30,03		WNW	1	Fine.
	2	0	42	54	30,15		WNW	1	Fine.
30	8	0	33	51	30,24		WNW	1	Cloudy.
	2	0	42	54	30,12		SSW	2	Cloudy.
31	8	0	46	52,5	29,72	0,297	SSW	2	Cloudy.
	2	0	47	56	29,57		SW	2	Fair.

1791.	Thermometer without.			Thermometer within.			Barometer.			Rain.*
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	53	31	42,1	58	46	52,9	30,58	28,18	29,56	1,937
February	52,5	30	41,1	57	48,5	52,5	30,48	29,16	29,94	0,873
March	55	31,5	44,3	61,5	48	55,6	30,67	28,90	30,20	0,716
April	64	41	51,9	64	55,5	60,0	30,11	29,08	29,77	1,460
May	67,5	39	53,1	61,5	55	58,1	30,37	29,53	30,02	0,794
June	80	47	61,3	70,5	57	63,0	30,22	29,39	29,93	0,332
July	78,5	32,5	62,6	68	61	64,2	30,24	29,44	29,89	2,194
August	78,5	50	64,9	72	62	67,1	30,52	29,65	30,06	0,824
September	77	43	59,5	71,5	58,5	62,5	30,33	29,52	30,09	0,482
October	62,5	34	48,9	61,5	52,5	57,8	30,46	28,89	29,69	2,027
November	52,5	25	43,6	59,5	48	54,5	30,28	28,76	29,68	2,527
December	48	21	36,7	56	45	50,0	30,38	28,90	29,64	1,124
Whole year			50,8			58,2			29,87	15,310

* On consulting other registers kept in and near London, it appears that the quantity of rain collected in the rain-gage of the Royal Society is remarkably deficient. Experiments are now making to determine the cause of this deficiency, and, if possible, its amount. In the mean time it was thought right to apprise the public of the fact, that no reliance may be placed on that part of the Meteorological Journal, till farther information has been obtained.